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THE PHYSIOLOGICAL CONTROL OF JUDGMENTS OF DURATION: EVIDENCE FOR A CHEMICAL CLOCK*[1]

From the Physiological Laboratory of Clark University

HUDSON HOAGLAND

One's ability to estimate time correctly is often surprising. It is frequently possible to waken oneself within a few minutes of a time decided upon before going to sleep (Brush, 10). Experiments involving the wakening of sleeping subjects and having them guess the time have shown an accuracy of ±15 minutes (Boring and Boring, 5). These facts, along with our normal waking estimations of duration, call for a timing process of considerable accuracy.

It has been suggested that the pulse or the respiration may serve as a clock which becomes correlated with our conventional time scale, but evidence for this is unconvincing. For an experimental approach to this problem cf. Goudriaan (23).

In recent years investigators have shown that rhythmic fluctuations of electrical potential occur in groups of cells in respiratory centers of the nervous system. These fluctuations produce bursts of nerve impulses over efferent nerve fibers. The basic activity has been shown to be quite independent of afferent control (Adrian and Buytendijk, 3; Adrian, 1; Bronk and Ferguson, 6). The relation of temperature to the rhythmic execution of creeping movements in poikilothermous animals, and of such rhythmic phenomena as the stridulations of crickets, earlier led Crozier and Stier (16) to suggest that the particular mechanism underlying each of these effects is of the nature of an irreversible chemical reaction in cells of the nervous system. The frequency of the emission of bursts of efferent impulses was assumed to be directly proportional to the velocity of the underlying reaction. It is interesting that similar suggestions were made by Crozier and Stier (16, 18) to account for rhythmic respiratory

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activity on the basis of the temperature analysis before direct discoveries of rhythmic fluctuation of electrical potentials within respiratory centers were made.

The multimodal frequency distribution curves of temperature characteristics found in the study of many biological processes (Crozier, 12, 13; Stier, 31) implies the existence of a limited number of catalytic substances involved in the control of rates of a wide variety of physiological mechanisms including the emission of repetitive discharges of impulses from nerve centers. The values of μ, the temperature characteristic or critical increment, for practically all known observations exhibit prominent modes at 8, 11, 12, 16, 18, 20, 22, 24, and 32 thousand calories. Specific catalytic substances have been assigned to a few of these magnitudes, such notably as 16,000 for iron catalysis in respiratory systems, 11,000 for hydroxyl ion catalysis, and 20,000 for hydrogen ion catalysis (cf. Stier, 31, p. 828, for summary).

The repetitive discharge of nerve impulses from the lateral-line receptors of fishes (Hoagland, 28), and the effect of temperature on the frequency of these impulses considered in the light of the foregoing facts, suggested the possibility of investigating the estimation of duration as a function of temperature. If our judgments of time depend upon an underlying chemical "master reaction," in, let us say, cells of the brain, modification of the internal body temperature might be expected to alter judgments of time intervals in a way consistent with the Arrhenius equation and might yield a significant temperature characteristic. Such a finding might even imply a specifically catalyzed, irreversible chemical mechanism controlling the consciousness of duration.

My wife, having fallen ill with influenza, was used in the first of several experiments. Without, in any way, hinting to her the na-

TABLE 1

Temperatures (°F) in order of recording	Actual seconds per counted minute	Means
103.0	34, 37, 39, 40	37.5
102.0	35, 37, 37, 35, 37, 36	36.0
99.6	44, 45, 48	46.0
99.0	44, 50, 47, 46, 53	48.0
100.0	44, 43, 47, 42, 42	43.5
97.4	52, 52, 51, 52, 54	52.0

ture of the experiment, she was asked to count 60 seconds to herself at what she believed to be a rate of 1 per second. Simultaneously the actual duration of the count was observed with a stop-watch. The reciprocal of the number of actual seconds occupied by the count was taken as a direct measure of the rate of the physiological process underlying the count.

Table 1 shows data of this experiment taken over a period of 48

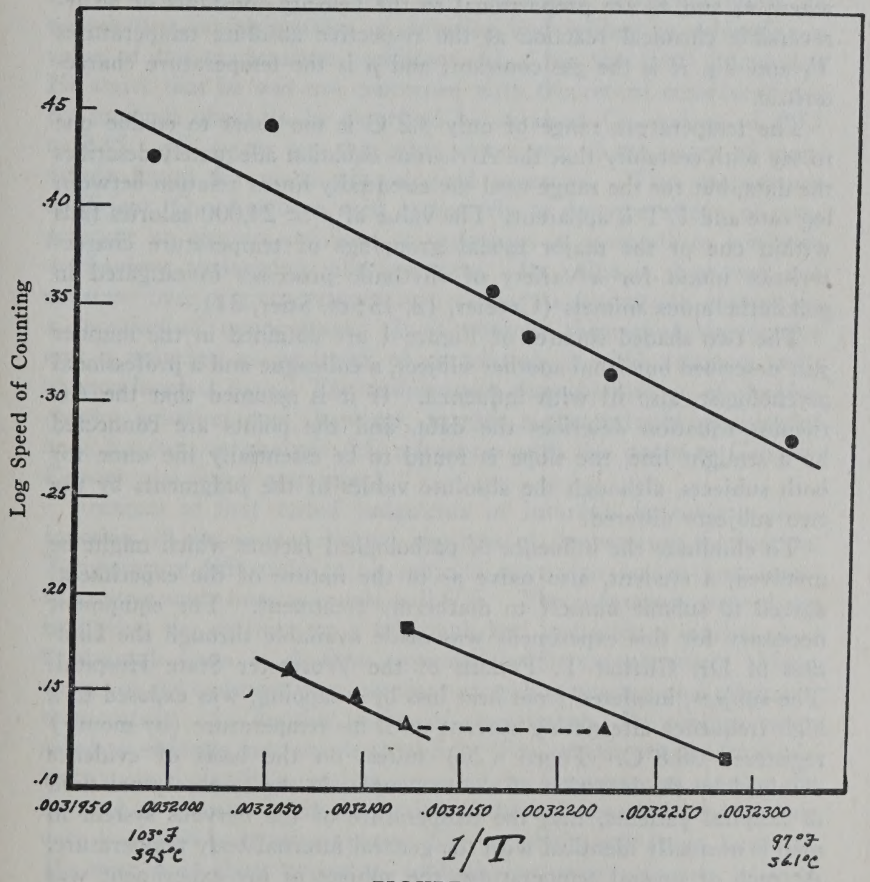

FIGURE 1

PLOT ACCORDING TO THE ARRHENIUS EQUATION OF ESTIMATIONS OF DURATION AS A FUNCTION OF THE INTERNAL BODY TEMPERATURE

Three experiments on different subjects are indicated. For discussion see text.

hours and covering a temperature range of 36.3°C to 39.5°C. The temperatures were taken by mouth with a clinical thermometer. Averages of at least five judgments were obtained at each of the experimental temperatures. The shaded circles of Figure 1 show the results plotted according to the Arrhenius equation,

$$\log_e \frac{k_1}{k_2} = \mu/R\ (1/T_2 - 1/T_1),$$

where k_1 and k_2 are proportional to the velocity constants of an irreversible chemical reaction at the respective absolute temperatures T_1 and T_2, R is the gas constant, and μ is the temperature characteristic.

The temperature range of only 3.2°C is too short to enable one to say with certainty that the Arrhenius equation adequately describes the data, but for the range used the essentially linear relation between log rate and 1/T is apparent. The value of $\mu = 24{,}000$ calories falls within one of the major modal groupings of temperature characteristics found for a variety of rhythmic processes investigated in poikilothermous animals (Crozier, 12, 13; cf. Stier, 31).

The two shaded squares of Figure 1 are obtained in the manner just described but from another subject, a colleague and a professional psychologist, also ill with influenza. If it is assumed that the Arrhenius equation describes the data, and the points are connected by a straight line, the slope is found to be essentially the same for both subjects, although the absolute values of the judgments by the two subjects differed.

To eliminate the influence of pathological factors which might be involved, a student, also naïve as to the nature of the experiment, agreed to submit himself to diathermy treatment. The equipment necessary for this experiment was made available through the kindness of Dr. Clifton T. Perkins of the Worcester State Hospital. The subject, insulated from heat loss by wrapping, was exposed to a high frequency alternating current until his temperature (by mouth) registered 38.8°C. Foged (20) states, on the basis of evidence obtained by the insertion of thermocouples in the cerebrospinal fluid of hospital patients, that the temperature of the nervous system in man is normally identical with the general internal body temperature. At each of several temperatures the subject of my experiment was asked to estimate intervals of one minute by the method already described. The triangles of Figure 1 show the results. The three

higher temperatures give points which, when connected, yield $\mu =$ 24,000 calories. The first point obtained at the lowest temperature is not on a line with the others of the plot. The subject was obviously nervous at the beginning of the experiment and his judgments were at first quite erratic. Five to eight judgments were obtained for each of the points.

After the completion of the foregoing experiments my attention was called to a very interesting paper by François (21), from Piéron's laboratory. He had been concerned with the effect of temperature on estimations of duration and wished to determine a value of the temperature coefficient, Q_{10}, for the time judgments. He states that he was not concerned with theoretical considerations. By methods about to be described he obtained Q_{10} values of 2.75 to 2.85. He points out that such values are of the order of magnitude found for many physiological processes. The temperature coefficient, Q_{10}, has been used extensively to describe effects of temperature on various activities of organisms in attempts to interpret mechanisms underlying vital processes. Its value is, however, not constant over a given temperature range, its magnitude varying as a function of temperature. It is without theoretical significance and is valueless as an index of correlation of vital processes with physicochemical data. The temperature characteristic, μ, of the Arrhenius equation does, however, furnish a theoretically meaningful basis for such an analysis (cf. references with this paper to Crozier, Crozier and Stier, and Stier).

François at first tested judgments of intervals in subjects as a function of the normal diurnal rhythms of temperature fluctuation. Temperature differences in his subjects, over nine- to ten-hour intervals, were sometimes as much as 0.8°C. The judgments were made by having the subjects tap a telegraph key, operating a marker on a kymograph drum, at rhythms requested by the experimenter. Mean values of the cadences were obtained at each maximum and minimum temperature. In another series of experiments the subjects were asked to estimate metronome rhythms. Ten experiments on two subjects showed clearly a mean increase in the frequency of tapping the key and an increase in the estimation of the cadence of the metronome with increased body temperature. The variability was large, however. He next used short exposures to high frequency currents to raise the temperatures of his subjects. In this way it was possible to test the judgments at two temperatures, a degree apart, within half an

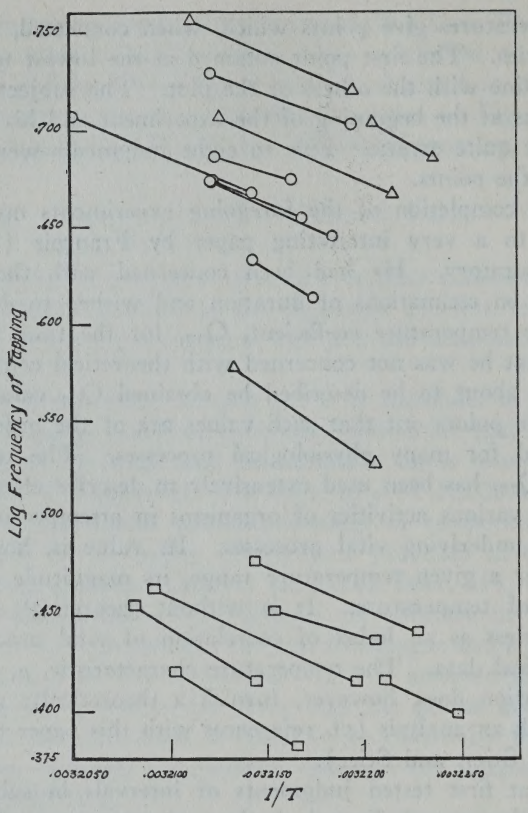

FIGURE 2

PLOT SIMILAR TO FIGURE 1 OF DATA OBTAINED BY FRANÇOIS (21) IN WHICH A DIFFERENT EXPERIMENTAL PROCEDURE WAS EMPLOYED FROM THAT USED IN OBTAINING THE DATA OF FIGURE 1

For discussion see text.

hour. With the diathermy technique much of the variability was eliminated in the key-tapping procedure, although it remained fairly high with the procedure in which the metronome was used. Twenty-one experiments were made with three subjects using the former procedure. These experiments showed consistently an increase in the rhythm of tapping with an increase in body temperature, yielding the values of Q_{10} mentioned above. Very little correlation was found between the pulse rate and the judgments. In some

experiments diathermy failed to increase the temperature but did increase the heart rate by as much as 20 per cent. Here, with the constant body temperature, the frequency of tapping was constant despite the increased pulse rate. The effect of diathermy treatment on the pulse was found to be very variable as compared to that on the rhythm of tapping.

Assuming that the Arrhenius equation describes these data, one may make a plot by connecting the mean high and low temperature-time judgment points by a straight line according to the equation. Figure 2 is such a plot in which the same large coordinates are used as in Figure 1. The open circles, triangles, and squares represent three different subjects. Each of François' experiments involving the tapping of rhythms is represented by two points and their connecting line. Sixteen experiments on the three subjects are recorded in the figure. The slopes are seen to be surprisingly constant although the absolute rates, as determined by the ordinates, vary considerably, not only for different subjects but for different experiments with the same individual. The person indicated by the squares shows uniformly faster rhythms of tapping than either of the others but the effect of temperature on the rhythms is similar for all. The

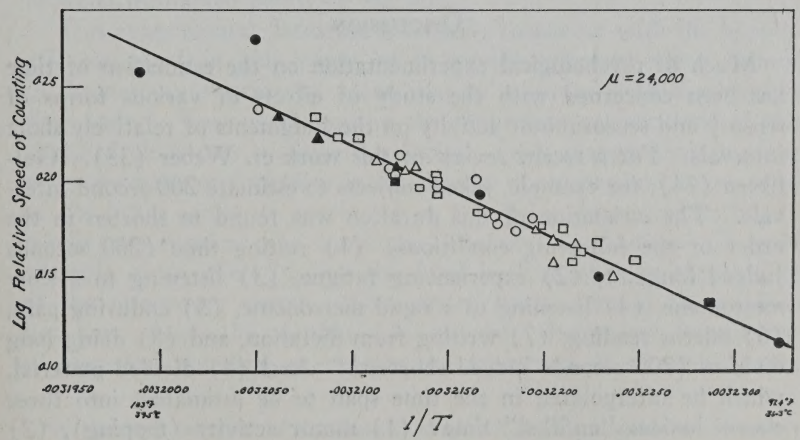

FIGURE 3

COMPOSITE PLOT OF DATA OF FIGURES 1 AND 2 SHOWING SIMILARITIES OF SLOPES (AND HENCE μ VALUES) FOR ALL OF THE EXPERIMENTS

For discussion see text.

person indicated by triangles shows very large variation in his absolute judgments of time from day to day; it is as if his physiological clock were capable of fluctuations in speed over gross intervals of time. The unique nature of the clock is indicated, however, by the constancy of the effect of temperature as indicated by the uniform slopes of the lines.

Figure 3 shows a combined plot of the data of Figures 1 and 2. This plot was made by tracing each curve of Figures 1 and 2 in such a way that, while the same abscissas were used, the differences in the absolute values of the ordinates are eliminated. The ordinate scale is of the same size as that previously employed but the tracing paper was moved up or down and the points are brought together along the y axis to show the essential similarity of slopes of all the lines. The composite plot of Figure 3 yields a value of $\mu = 24{,}000$ calories.

These results are interesting since they imply the existence of a unitary chemical process probably irreversible in nature and perhaps catalyzed in a specific way. The fact that my results agree so well with those calculated from the data of François, of whose experiments I was unaware until after my calculations were completed, is worthy of note, especially since we employed different modes of evaluating the speed of judgments of time.

Discussion

Much of psychological experimentation on the estimation of time has been concerned with the study of effects of various forms of sensory and sensorimotor activity on the judgments of relatively short intervals. For a recent review of this work cf. Weber (33). Gulliksen (24), for example, asked subjects to estimate 200-second intervals. The estimation of this duration was found to shorten in the order of the following conditions: (1) resting time (200 seconds judged longest), (2) experiencing fatigue, (3) listening to a slow metronome, (4) listening to a rapid metronome, (5) enduring pain, (6) mirror reading, (7) writing from dictation, and (8) doing long division (200 seconds judged shortest). Axel (4) divided material, which he interpolated in the time span to be estimated, into three classes besides "unfilled" time: (1) motor activity (tapping), (2) sensorimotor activity (canceling particular numbers of a series), and (3) mental activity of a more complicated nature. He found that the level of behavior which is in operation markedly affects the estimations of short intervals. The higher level "fillings," such as

"analogies" and "number series completion," gave underestimations. "Unfilled" time he found to be estimated about the same as that of intervals occupied by tapping and he regarded this as evidence that there is no definite line of demarcation between estimates of "filled" and "unfilled" times.

Not only does fairly complex activity alter judgments of time, but phenomenal velocity of perceived movement (phi-phenomenon, tau effect, etc.) varies with changes of the patterns of sensory stimulation. Brown (7, 8) has pointed out that phenomenal velocity is equal to $\frac{\text{phenomenal space}}{\text{phenomenal time}}$. Most cases of the apparent movement of successively exposed objects in a visual field are conditioned by the total field stimulated. Brown finds that the phenomenal velocity of movement depends primarily upon variations in phenomenal time rather than upon variations in phenomenal space. He concludes that variations in phenomenal time are not profitably to be regarded as illusions but are essentially normal continuous phenomena correlated with changes in the structure of the field of movement.

Helson and King (25) have found a close interrelation of temporal and spatial factors involved in the estimation of distances between stimulated points on the skin.

This experimental literature is entirely consistent with the hypothesis suggested in this paper of a specific chemical clock located in the nervous system and furnishing its possessor with a subjective time scale.[2] Various changes in the immediate environment of this chemical clock may modify its velocity. Such changes may be those of temperature or variations in the activity of sensory and motor areas of the brain acting to accelerate or to inhibit (experimentally primarily the former) the continuous chemical mechanism. A good example of an effect of just this kind is illustrated by the activity of the respiratory center. The excised brain stem of the goldfish has been shown by Adrian (3) to emit rhythmical fluctuations of electrical potential at exactly the frequency of normal, resting

[2]A classical example of a chemical clock which has been of considerable value to geologists in estimating the age of rock strata is, of course, the irreversible decomposition of radium, which follows the simple exponential mass action kinetics of a monomolecular reaction. The decompositions of radioactive elements are unique among chemical reactions in being entirely unaffected by temperature.

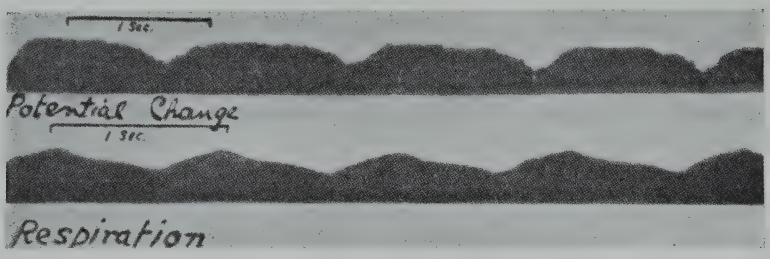

FIGURE 4

RHYTHMIC POTENTIAL CHANGES IN THE ISOLATED BRAIN STEM OF THE GOLDFISH COMPARED WITH RECORD OF GILL MOVEMENTS IN AN INTACT FISH

From Adrian (2, p. 82), by permission of the University of Pennsylvania Press.

opercular breathing rhythms of intact fishes (cf. Figure 4 reproduced from Adrian's book, *The Mechanism of Nervous Action*).

These variations in potential produce synchronous bursts of efferent impulses to the respiratory musculature. Crozier and Stier (17) had earlier shown that the opercular rhythms of goldfish obey the Arrhenius equation when they plotted the logarithm of the frequency of opercular beats as a function of the reciprocal of the absolute temperature (cf. Figure 5). A temperature characteristic

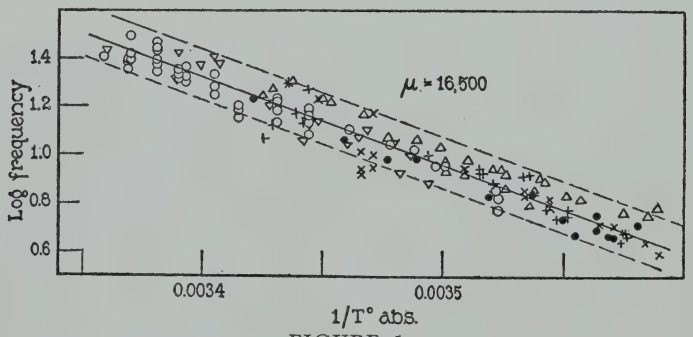

FIGURE 5

THE FREQUENCY (100 DIVIDED BY TIME FOR 10 MOVEMENTS) OF RHYTHMIC OPERCULAR MOVEMENTS IN SIX GOLDFISH (DIFFERENT SYMBOLS) AT DIFFERENT TEMPERATURES, WHEN THE PECTORAL FINS ARE NOT OBVIOUSLY MOVING

The four most divergent points are single observations, the majority of the others, averages of four or more closely concordant readings. The critical increment is 16,500 calories. (The observations pertaining to one individual [black circles] are divided by 1.34.)

From Crozier and Stier (17, p. 700), *J. Gen. Physiol.*

of 16,500 calories was obtained, which indicates that the rhythms are probably produced by a continuous respiratory, oxidative process within the center catalyzed by iron. Measurements of the metabolism of goldfish as a function of temperature have been made by Ege and Krogh (19). The velocity of oxygen consumption also yields a μ of 16,500 calories.

The respiratory center is known to be very sensitive to changes of hydrogen ion (or CO_2) concentration in its tissues, even very slight decreases in the normal pH of the blood, such as are produced by mild muscular exercise, accelerating respiratory movements (cf. Gesell, 22). It is not unreasonable to suppose that chemical changes locally produced in the brain by excited groups of neurons would increase the velocity of our hypothetical clock, making time appear to pass more rapidly when we are active. This effect might be especially apparent when the activity involves extensive excitation of higher levels of the central nervous system. The band-like spread of the data on the frequency of opercular beats in Figure 5 has been accounted for by Crozier and Stier (17) in a way similar to this. To quote from their paper:

> "The nature of the variation of the frequency of opercular movements at constant temperature, requires brief consideration. The pertinent facts are these: the latitude of variation tends to be a well defined constant fraction of the mean frequency, at each temperature; the incidence of pectoral [fin] movements *accelerates the opercular rhythm* [italics mine] but there is a clear indication that the central discharge controlling the movements is thereby merely pitched at a new level,—for the *critical increment is sensibly the same;* [italics mine] a similar result is obtained under moderate starvation. . . . A full theory of these effects awaits further testing but it is believed that they permit the assumption of definite views regarding the mechanism of variability of central nervous processes. If it be supposed that the reactions controlling discharge from the respiratory center are specifically synaptic in locus, the effective mass of a governing catalyst might be varied according to the influx of nerve impulses from other parts of the central nervous system. Changes of this kind would abruptly alter the frequency of the observed activity without changing the critical increment."

The critical increment, or temperature characteristic, of opercular rhythms may be altered in goldfish. Crozier and Stier (18) accom-

plished this by exposing fishes previously ascertained to yield a μ of 16,500 calories to a temperature of 25° C for three hours in water with an oxygen content of about three cc. per liter. A new increment of 8,300 calories was then obtained which lasted for some hours after the treatment, indicating a temporary shift in the catalytic mechanism of the master reaction controlling respiration.

Figure 6, reproduced from the *Journal of General Physiology,* shows data obtained with two fishes yielding $\mu = 8{,}300$ calories. This figure also illustrates the possible effect of central nervous activity involved in the fin movements on the absolute rate of the reaction. The absolute rhythms for fishes 13 and 14 of Figure 6 differ, but the value of μ is the same for both. The typical effect of the motor activity (fin movements) of fish No. 13 in accelerating the master reaction controlling its respiratory movements offers a

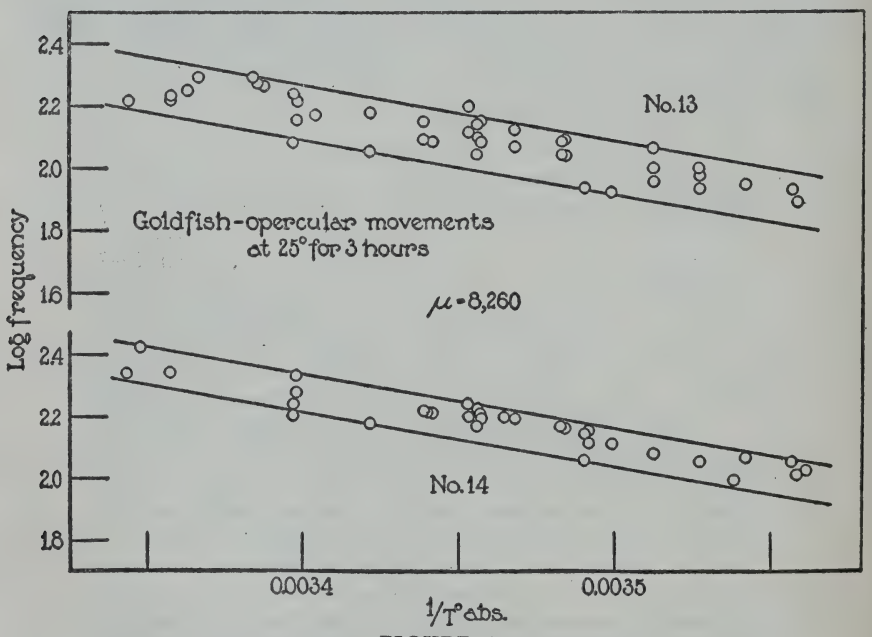

FIGURE 6

FREQUENCY OF OPERCULAR MOVEMENTS IN TWO GOLDFISH FOLLOWING THREE HOURS' EXPOSURE TO A TEMPERATURE OF 25° C

The plotted points are single observations. No. 13 showed constant gentle movements of the pectorals; No. 14 was quiet, with no fin movements. From Crozier and Stier (18, p. 552), *J. Gen. Physiol.*

striking parallel with the possible acceleration of our hypothetical chemical clock by sensorimotor activity. The similarities between Figures 1, 2, 3 and Figures 5 and 6 are self-evident, and can best be interpreted on the assumption that somewhat specific chemical mechanisms release in one case the rhythms of counting, and in the other case, the fishes' respiratory movements. The rhythms in both cases are subject to a similar type of modification by sensorimotor activity.

In both my own experiments and those of François sensorimotor conditions were relatively constant, corresponding to a "resting condition" in my experiments or to tapping "filled" time in those of François. Axel (4) has shown that these two temporal "fillers" are essentially equivalent. Fluctuations in estimates from day to day by the same subject in his absolute estimates of time occurred in the experiments, and this is to be expected, since many possible internal variables could be controlled only roughly. The constancy of the value of $\mu = 24{,}000$ calories, however, indicates that a unitary chemical mechanism is probably basically determining the judgments. The fact that this value of μ is characteristic of many vital processes (cf. Crozier, 13) suggests that the chemical mechanism basically involves an irreversible process, catalyzed by a specific agent, as yet unidentified, but common to many protoplasmic systems.

The value of $\mu = 24{,}000\pm$ calories has been obtained in a number of cases. It is usually found to be associated with cell respiration. For example, Tang (32) finds $\mu = 24{,}000$ calories for the production of CO_2 by germinating seeds of *Lupinus albus*. Oxygen consumption in these tissues yields $\mu = 16{,}600$ calories. Crozier and Stier (18), using data of Pütter on the oxygen consumption of leeches, finds $\mu = 24{,}000$ calories when the oxygen tension is only $130\pm$ mm. Hg. At higher oxygen tensions ($400\pm$ mm. Hg.) a value of $\mu = 7{,}900$ calories was obtained, indicating the existence of two possible catalytic master reactions either of which may control the rate of oxygen uptake depending on the oxygen tension. Observations by Stier on the heart beat of the garter snake as a function of temperature were found to yield $\mu = 23{,}500$ calories (Crozier, 13). Values of $\mu = 24{,}000$ calories have been observed in connection with certain growth processes (Crozier, 14). This same value has occasionally been obtained for frequencies of pharyngeal breathing rhythms in frogs. A value of 24,200 calories has been found for pharyngeal breathing rhythms in toads below 15°C. The

most common values of μ determined for a wide variety of respiratory processes and often found associated in the same animal on different sides of critical temperatures are, in order of their prominence, 16,000±, 11,000±, 8,000±, and 24,000± calories (Crozier, 12).

The molecular significance of μ has been strikingly demonstrated by some experiments of Pincus (29), who tested the inheritance of temperature characteristics determined for the respiratory movements of very young mice of highly inbred strains and their crossed progeny. By using mice, only a few days old, before the development of their temperature regulatory mechanisms, it was possible to obtain μ values for these mammals over the range of 15 to 25°C. Young mice of one strain gave consistently μ = 24,000± calories or, less frequently, 28,000± calories for frequencies of respiratory movements. Those of another inbred strain gave values of 34,000±, or, less frequently, 14,000± calories. The F_1 hybrids of these two strains and the backcross generations to either parent strain "exhibited only those four values exhibited in the parent strain and none other. One may, therefore, speak of the inheritance of the value of the constant μ, but the inheritance shows in this instance no Mendelian behavior."

The experiments on the estimation of time described above deal only with short durations. It is possible that other mechanisms of a cyclic nature are involved in estimates of longer intervals. Boring and Boring (5) found a variety of possible cues used by subjects who were awakened during the night and asked to estimate the time. Conscious internal cues such as restlessness, fatigue, "clearness of consciousness," sensations involved in excretory and digestive functions apparently served as a basis for the time judgments. These are processes of a complex physiological nature, but are essentially rhythmically recurrent, and may, therefore, serve as clocks.

In this connection it might be worth pointing out that considerable evidence exists for believing that cyclic periods of alternate rest and quiescence in mammals, i.e., periods of "spontaneous" activity, are controlled by specific master chemical reactions which serve to regulate the periods (Stier, 30).

The phenomenon of tonic immobility (animal hypnosis) is an interesting special case of "spontaneous" activity. The durations of cycles of reflexly produced tonic immobility have been shown in lizards, *Anolis carolinensis* (Hoagland, 26, 27), to depend prob-

ably upon the rate of decomposition of two autacoid substances released reflexly by the immobilizing stimulus. The animal remains immobile until the concentration of the inhibitory hormones in the circulation has declined to subthreshold levels. The application of the Arrhenius equation to reflexly induced cycles of tonic immobility in lizards yielded two independent temperature characteristics. A somewhat similar humoral control of the duration of tonic immobility cycles has been demonstrated by Crozier (15) in the isopod (sowbug) *Oniscus asellus* Linn. The temperature analysis of the rate of decline of a single controlling humoral agent is here somewhat complicated, but it is interesting that between the ranges of 5° to 16°C a value of $\mu = 24{,}000$ calories was obtained when the logarithms of the reciprocals of the durations of the cycles (proportional to the rate of the basic process) was plotted against the reciprocals of the absolute temperatures.

Excretory and digestive cycles which furnished the basis of temporal judgment for some of the subjects in the experiments of Boring and Boring may be regarded basically as "spontaneous" activities as this term is used by Stier. Temperature characteristics for such cycles in very young mammals, before the development of mechanisms controlling the body temperature, indicate that the cycles may be dependent upon specifically catalyzed master chemical reactions different from those previously discussed (Stier, 30). These facts lend support to the notion that reactions may be involved in the estimation of long durations which may be different from the reaction indicated in this paper (corresponding to $\mu = 24{,}000$ calories) which appears to serve as a basis for judging short temporal intervals.

It is tempting, but probably quite unfruitful, to speculate concerning the seeming parallel between the unidimensional, irreversible character of subjective time and the notion of a continuous, irreversible chemical clock in the brain.

We have dealt so far with the physiology of short durations of the order of a minute, and with that of estimations of longer durations of the order of hours. Let us now consider factors involved in the rough estimation of very long durations of the order of one's life span.

Physiological time, from this last point of view, has been interestingly discussed by Carrel in 1931. He points out that physiological and psychological time do not appear to pass at a constant rate through physical time. In early youth physical time seems much

longer than in old age when it appears to pass rapidly. Physiological time is determined by a flux of processes essentially irreversible in nature which progress during the life span. Growth, puberty and menopause, lowering of basal metabolism, modification of skin and hair appear to mark the passage of physiological time. Carrel considers in his paper several quantitative measures of this sort of physiological time. Tissue culture preparations obtained from organisms of different ages show marked differences in residual growth energies when placed in non-nutrient isotonic saline solutions. In embryonic tissues this residual growth energy is greater than in older tissues and declines progressively with age.

The rate of healing of wounds (cicatrization) is a function of the age of patients. An experimental formula proposed by du Noüy describes the relation. If the constants of the equation are known one can determine the physiological age of the patient.

Carrel points out that growth is related to simultaneous changes in the body fluids. The rate of aging of tissue culture cells is entirely different in a drop of fluid and in a flask of large volume. The production of metabolites and toxins is the important limiting factor since cells are potentially immortal. The closed internal environment of higher organisms is kept relatively constant by the action of lungs, kidneys, intestines, and various mechanisms of homeostasis, but nevertheless continuous irreversible changes go on throughout life.

With the decline in the growth energy of tissues are found parallel changes in the body fluids. The development of cultures of fibroblast cells is inhibited, for example, by blood serum. The ratio of the area of a colony of cells living in serum to that of a similar colony living in saline solution is called the growth index of the serum. The serum of a dog a few days old has no inhibitory effect as indicated by the growth index. The growth index of the serum decreases most rapidly during the first few months of life. In old age the decrease from year to year is hardly perceptible. The growth index seems to depend upon changes of serum proteins and lipoids (primarily the latter) with age. Carrel suggests that these changes are probably responsible for the decrease in the value of the growth index and also for the change in the index of cicatrization of wounds as described by du Noüy's formula. I should like to point out that it has been shown (cf. Bruen, 9) that human basal metabolism, as

a function of age, follows an exponential relation. This relation is highly suggestive of du Noüy's cicatrization formula.

Carrel concludes that physiological time is determined by a succession of irreversible changes in the system cells-medium and can be measured and expressed in special units. It varies progressively in a way that physical (astronomical) time does not.

To illustrate the apparent speeding-up of physical time in old age, he suggests the analogy of two trains running in the same direction on parallel tracks, one representing physical time and the other physiological time. At first the trains are running at equal speeds and then one (physiological time) slows up. The other continues at a uniform speed so that to passengers on the decelerating "physiological train" it appears to go faster.

Conclusions

Psychological time seems to depend directly upon the velocities of certain definite chemical processes, the psychological and physiological events forming different aspects of the same thing. Measurements of the estimations of short durations indicate the existence of a master chemical clock of a specific nature, possibly the slowest of the series of irreversible processes involved in the respiration of certain parts of the brain. Longer intervals of time appear to be judged in terms of the velocities of other master chemical reactions (cf. Stier, 30) which determine cyclic diurnal rhythms. Large scale conceptions of duration evidently depend upon slowly accumulating irreversible effects in the internal environment composing the body humors (Carrel, 11).

Several procedures are now being considered as a means of approaching further the general physiology of time. Thanks to Dr. Perkins of the Worcester State Hospital, more work with the diathermy technique is to be undertaken. In addition, by exposure to cold it is possible to reduce the body temperature considerably below normal; experiments in a low-temperature room promise to yield an interesting extension of the analysis of the estimation of short durations.

Mr. Edward Kemp, a graduate student in psychology at Clark, suggested to me the possibility of developing a conditioned trace reflex in a poikilothermous animal and studying the effect of temperature on the time interval (trace) between the conditioned stimulus and the response. Such experiments are now under weigh.

Variations in basal metabolism artificially induced by the feeding and injection of suitable hormones may be found to produce interesting experimental alterations in estimations of duration.

Summary

1. Judgments of short durations vary with the internal body temperature.

2. The judgments of duration appear to be described by the Arrhenius equation for the short temperature range of 3.2°C. A temperature characteristic of 24,000 calories is found for my own data and for those of François (21). These sets of data were taken independently and by the use of different experimental procedures.

3. The absolute judgments of time by the same subject may differ from day to day, but the effect of temperature on the time judgments is constant. It is suggested that judgments of short duration may depend immediately on the velocity of a particular continuous chemical reaction (clock) in the nervous system, probably irreversible in nature, and catalyzed in a particular manner corresponding to $\mu = 24{,}000$ calories.

4. Factors other than temperature, such as sensorimotor activity, may modify the speed of the chemical clock, and hence our subjective judgments of time.

5. Physiological events which may determine the estimation of long durations are considered, and conclusions as to the mechanisms determining psychological time are discussed.

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LE CONTRÔLE PHYSIOLOGIQUE DES JUGEMENTS DE LA DURÉE: ÉVIDENCE D'UN CHRONOMÈTRE CHIMIQUE

(Résumé)

Les judgments des courtes durées varient selon la température interne du corps et il semble qu'on puisse les décrire par l'equation d'Arrhenius dans la rangée de 3,2° C. Une température caractéristique de 24,000 calories se trouve pour mes données et pour celles de François. Ces groupes de données ont été indépendamment trouvés en employant des processus expérimentaux différents. On suggère que les jugements de la durée dépendent de la vélocité d'une réaction chimique particulière (chronamétre) dans le systéme nerveux, laquelle ne se renverse pas probablement et est catalysée d'une manière spéciale qui correspond à μ=24,000 calories. L'activité sensorielle motrice accroît, paraît-il, la vitesse absolue du chronométre chimique sans changer la valeur de μ.

L'effet de la température sur le sens du temps se montre très semblable à son effet sur certains processus de l'origine nerveuse centrale chez les animaux de sang froid. On a trouvé que l'activité motrice modifie ces processus d'une manière qui ressemble à la manière dont elle modifie le sens du temps. La valeur de μ=24,000 calories s'applique à une variété de systèmes biologiques et est généralement associée aux phénomènes respiratoires des cellules.

On discute les facteurs physiologiques engagés dans l'estimation des longues durées.

Hoagland

DIE PHYSIOLOGISCHE BEHERRSCHUNG DER DAUER-URTEILE: BEWEISE FÜR DAS VERHANDENSEIN EINER CHEMISCHEN UHR

(Referat)

Urteile über kurze Zeitintervalle varieren mit der inneren Körpertemperatur und scheinen im Umfang der Experimente von 3.2°C durch die Arrheniusgleichung bestimmt zu sein. Für meine eigenen Angaben wie die von François lässt sich eine Temperatur feststellen, die 24000 Kalorien entspricht. Diese Angabengruppen wurden unabhängig von einander und durch den Gebrauch verschiedener Experimentalvorgänge aufgenommen.

Es drängt sich uns die Vermutung auf, dass Dauer-Urteile von der Geschwindigkeit einer besonderen chemischen Reaktion (Uhr) im Nervensystem abhängen, die ihrer Natur nach nicht umkehrbar ist und in einer besonderen Weise, die μ=24000 Kalorien entspricht, katalysiert wird. Offenbar vermehrt sensorische Tätigkeit die absolute Geschwindigkeit der chemischen Uhr ohne den Wert des μ zu ändern.

Es lässt sich feststellen, dass die Wirkung der Temperatur auf den Zeitsinn ähnlich ist ihrer Wirkung auf gewisse Prozesse des Zentralen Nervensystems Kaltblütiger Tiere. Es zeigt sich, dass motorische Tätigkeit diese Prozesse in ähnlicher Weise verändert, wie sie den Zeitsinn verändert. Der Wert μ=24000 Kalorien ist charakteristisch für eine Reihe biologischer Systeme und wird allgemein Atmungserscheinungen in Zellen zugeschrieben.

Physiologische Faktoren, die in der Schätzung langer Zeitintervalle mitspielen, werden besprochen.

HOAGLAND

THE ERROR FUNCTION IN MAZE LEARNING*

From the Psychological Laboratories of the University of Chicago

L. L. THURSTONE

The object of this paper is to describe a method of dealing with maze-learning data so as to reveal functional continuities that are not readily discovered by the customary methods of analyzing maze data. The new method will enable us to estimate the learning ability of the subject even though he has not completely mastered the maze, to estimate the number of errors that may be attributed to exploration rather than to learning, to express quantitatively the relative difficulties of several mazes even though none of them are completely mastered, and to estimate the number of errors that would be required for complete mastery of the maze.

Consider first the relation between errors and trials as represented in Figure 1. This diagram represents the performance of animal No. 4 on Maze III in an experimental study by Lashley (1). The number of errors in each trial is here plotted against the number of the trial. In general, it is of course apparent that the frequency of

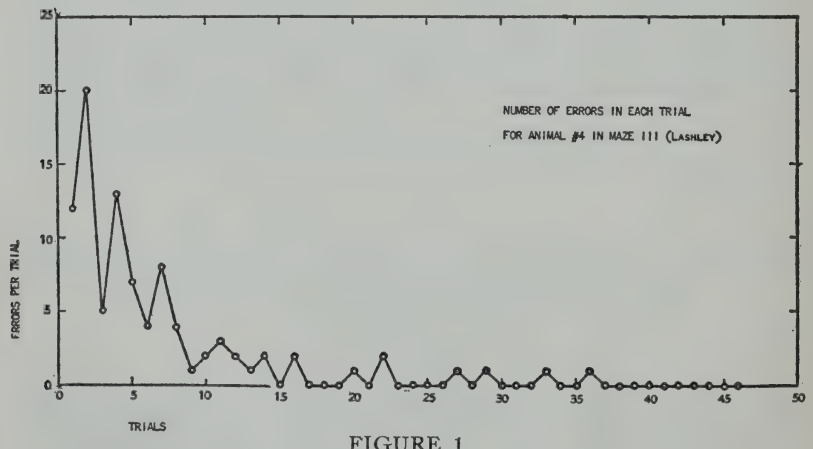

FIGURE 1

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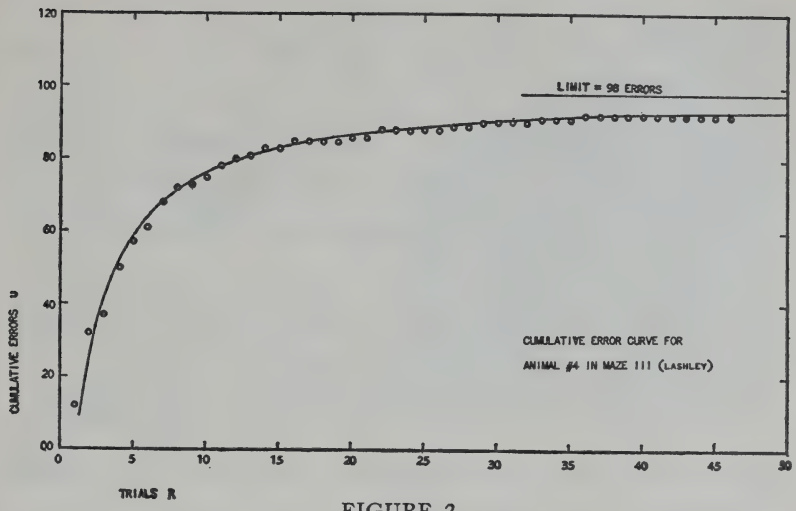

FIGURE 2

errors is reduced with increased practice, but no one would attempt to fit an error function on Figure 1 because of the extreme irregularities of the performance. In order to reveal the functional continuity in data of this kind it is a useful trick to plot the errors in cumulative form. This has been done in Figure 2 and it is here immediately apparent that we are dealing with a function which can be described algebraically at least with a fair degree of approximation. Inspection of these two figures shows that the zigzags which obscure the function in Figure 1 are eliminated in Figure 2. A smooth and continuous curve can be drawn in the second figure so as to represent the data. It is simply the integral of the curve in the first figure and if an equation can be fitted to Figure 2, then the derivative would be the best-fitting curve for Figure 1. It is in this manner that we shall determine the equation of the error function in maze learning. This device of plotting cumulative errors against trials should prove to be useful rather frequently in dealing with erratic learning records.

The present study of the error function will be related as far as possible to the writer's previous studies of the learning function. It is then necessary first to consider the different variables that are involved in the former learning function and in the present error function. The equation of the learning function which has been developed in a previous paper is as follows

$$\frac{2p-1}{\sqrt{p-p^2}} = \frac{kt}{\sqrt{m}} + z_0 \qquad (1)$$

This was equation 27 in the previous study (2). In this equation the principal variables are p and t. Practice is measured in terms of time, t, and proficiency is measured in terms of the probability, p, that an act initiated at any moment of time will be counted as successful. This measure of proficiency is proportional to the number of successful acts per unit time.

The principal variables in the error function are the number of errors, e, which the subject makes in a trial and the ordinal number, R, of that trial. This is the function represented in Figure 1. In Figure 2 we have as the principal variables the total number of errors, u, which the subject has made in the first R trials. These two sets of variables can be unified in that equation 1 above may be transformed into one that will describe the cumulative error function of Figure 2.

Our first step in making the transformation is to set the origin for the learning function of equation 1 at the median point at which the task is exactly half mastered. This will be done in order that the constant z_0 may vanish and thereby simplify the transformation. This origin for time serves our purposes as well as any other because it is in any event essentially arbitrary. Then

$$\frac{2p-1}{\sqrt{p-p^2}} = \frac{kt}{\sqrt{m}} \qquad (2)$$

This equation may be rewritten explicitly for p so that it takes the form

$$p = \tfrac{1}{2} + \frac{t}{2\sqrt{\frac{4m}{k^2} + t^2}} \qquad (3)$$

This learning function is shown graphically in Figure 3. Instead of the relation between p and t, we are now looking for the relation between the cumulative errors, u, and the number of trials, R.

In making the transformation we shall make two psychological assumptions. We consider that learning begins at t_0 in Figure 3. The ordinates represent amount of successful output per unit time.

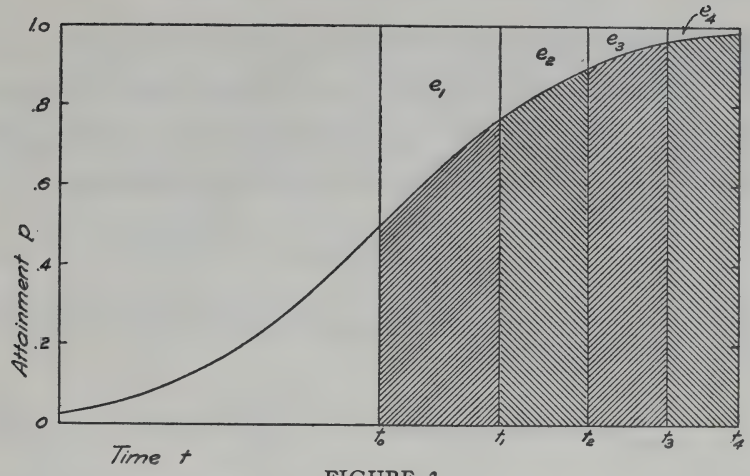

FIGURE 3

Now, the complete route through the maze to the food box may be taken to correspond here to unit output as far as the scoring of maze performance is concerned. If acceptable output be designated w, then it is clear that

$$dw = pdt \tag{4}$$

but since the number of runs R through the maze is a direct measure of acceptable output, disregarding the errors, we have

$$dR = pdt \tag{5}$$

in which a multiplying constant might be used to agree with any arbitrary units of measurement for w and for R.

In Figure 3 we have represented several successive sections of equal output by cross hatching. For example, the cross-hatched area between t_0 and t_1 represents the output required to complete one run in the maze to the food box. During this run certain errors are made and this number is e_1. Since at any moment of time the probability is p that an act will be successful, we have

$$\int_{t_0}^{t_1} pdt = \int_{t_1}^{t_2} pdt = \int_{t_2}^{t_3} pdt = \text{constant} = \text{output for each complete run through the maze.} \tag{6}$$

Since the value of p increases with effective or actual practice time, we shall expect the successive complete runs to require less and less time. The successive cross-hatched areas in Figure 3 are of the same

area, becoming taller with increase in p but narrower to show reduction in effective running time per trial with increased practice.

Since the probability is p that an act initiated at any moment of time will be successful, we have of course that $q = (1—p) =$ the probability that the same act will be an error or waste of effort. Then the number of errors in the first trial will be designated

$$\int_{t_0}^{t_1} qdt = ae_1 \tag{7}$$

and for any other trial the total number of errors will be designated in the same manner, so that

$$\int_{t_1}^{t_2} qdt = ae_2, \int_{t_2}^{t_3} qdt = ae_3, \ldots \int_{t_{n-1}}^{t_n} qdt = ae_n \tag{8}$$

in which a is an arbitrary constant which depends only on the relative units of measurement of time and of errors.

The successful output is constant for each run since the unit task is to run through the maze and to reach the food box. But the errors made in each run are not constant. They are some decreasing function of time.

Since q is the probability that an act that is initiated at any moment will be ineffective, it is clear that this is also the rate at which the cumulative error sum is increased per unit time. It was assumed in the previous study that the acts are initiated at a constant rate. Then

$$a\frac{du}{dt} = q \tag{9}$$

in which a is an arbitrary constant which depends only on units of measurement.

In Figure 3 the successive values of e_1, e_2, e_3 . . . e_n are represented by the non-sectioned areas so labeled. Each one of these values is the expected number of errors per trial, i.e., per unit output.

The two psychological assumptions are then

$$p = \frac{dR}{dt} \tag{5}$$

$$q = a\frac{du}{dt} \tag{9}$$

It is by means of these two psychological assumptions that we shall find the relation between trials, R, and cumulative errors, u, so that our equation in u and R shall be consistent with the learning function in equation 1. The success with which that equation has been verified on several learning hypotheses, such as the 3/2 power law for learning time, led the writer to the hope that it might also here make good in a different learning problem.

From equations 3 and 5, we have

$$\int dR = \int p\,dt \tag{10}$$

$$= \int \left[\tfrac{1}{2} + \frac{t}{2\sqrt{\frac{4m}{k^2} + t^2}} \right] dt \tag{11}$$

$$= \tfrac{1}{2} \int dt + \tfrac{1}{2} \int \frac{t\,dt}{\sqrt{\frac{4m}{k^2} + t^2}} \tag{12}$$

$$R = \frac{t}{2} + \frac{1}{2}\sqrt{\frac{4m}{k^2} + t^2} - c_1 \tag{13}$$

The value of q can be expressed as follows

$$q = (1 - p) = 1 - \left[\frac{1}{2} + \frac{t}{2\sqrt{\frac{4m}{k^2} + t^2}} \right] \tag{14}$$

$$= \tfrac{1}{2} - \frac{t}{2\sqrt{\frac{4m}{k^2} + t^2}} \tag{15}$$

Substituting 15 in 9, we have

$$a\int du = \int q\,dt \tag{16}$$

$$= \int \left[\tfrac{1}{2} - \frac{t}{2\sqrt{\frac{4m}{k^2} + t^2}} \right] dt \tag{17}$$

$$au = \frac{t}{2} - \frac{1}{2}\sqrt{\frac{4m}{k^2} + t^2} - c_2 \tag{18}$$

But by 13 and 18 we have

$$R + c_1 = \frac{t}{2} + \frac{1}{2}\sqrt{t^2 + \frac{4m}{k^2}} \tag{19}$$

$$\text{and } au + c_2 = \frac{t}{2} - \frac{1}{2}\sqrt{t^2 + \frac{4m}{k^2}} \tag{20}$$

When $R = 0$, then $t = 0$, $u = 0$, and hence

$$c_1 = \frac{\sqrt{m}}{k} \tag{21}$$

and

$$c_2 = -\frac{\sqrt{m}}{k} \tag{22}$$

Therefore

$$c_1 - c_2 = \frac{2\sqrt{m}}{k} \tag{23}$$

$$c_1 + c_2 = 0 \tag{24}$$

Adding 19 and 20,

$$t = R + au + (c_1 + c_2) \tag{25}$$

By 24

$$t = R + au \tag{26}$$

Subtracting 20 from 19

$$\left[t^2 + \frac{4m}{k^2} \right]^{\frac{1}{2}} = R - au + (c_1 - c_2) \tag{27}$$

By 23

$$\left[t^2 + \frac{4m}{k^2}\right]^{\frac{1}{2}} = R - au + \frac{2\sqrt{m}}{k} \tag{28}$$

In order to eliminate t, which we do not want in the relation between u and R, we substitute 26 in 28. Then

$$\left[(R + au)^2 + \frac{4m}{k^2}\right]^{\frac{1}{2}} = R - au + \frac{2\sqrt{m}}{k} \tag{29}$$

After expanding and simplifying, this equation reduces to the form

$$u = \frac{\sqrt{m}}{ak} - \frac{\sqrt{m}}{k} \cdot \frac{u}{R} \tag{30}$$

Equation 30 is the desired relation between cumulative errors, u, and trials, R. It has been written so as to be consistent with the learning function in equation 1 and the transformation from 1 to 30 has been made by the psychological assumptions represented by equations 5 and 9.

It can readily be seen that equation 30 is linear in u and u/R, which is a fortunate circumstance because this enables us to determine the two constants $\frac{\sqrt{m}}{ak}$ and $\frac{\sqrt{m}}{k}$ by rectification. If we plot values of u against the corresponding values of u/R, then the plot should be linear unless we are dealing with an appreciable number of exploratory errors which are to be described. Since the slope and intercept can be determined graphically, we can find the ratio of the two constants above, but the exact values of k and m cannot be ascertained from any single learning curve. This is to be expected since any particular speed of learning may be accounted for either by a high learning ability, k, or by a low degree of complexity of the task, m. However, these two constants can be separated by considering the records of several subjects who have learned several tasks by methods that were described in a previous paper (2).

There are two limitations to equation 30 that must be considered before turning to the data for verification. These concern the heterogeneity of the task and the exploratory errors of the animal. Of these two difficulties, the exploratory errors may be the more serious.

We shall ignore the heterogeneity of the task since we have no adequate scheme of evaluating it. Since the equation applies quite satisfactorily on the assumption of homogeneity of task, we shall proceed with this assumption except to note the effect that is to be expected by the degree of heterogeneity that may be involved in maze data. The effect on the cumulative errors should be that their frequency should be greater than expected in the earlier part of the learning and less than the expected frequency in the later part of the learning. In other words, we should expect the heterogeneity of the task to make the experimental curve of cumulative errors rise more rapidly at first and less rapidly in the later stages of learning than would be expected by equation 30. Since this is in effect a recognition of unexpected frequency of errors in the earlier stages of learning, the effect of heterogeneity will be similar to the effect of exploratory errors, which is also more noticeable in the early stages of learning than during its later stages.

It is possible to distinguish between two kinds of errors, namely, exploratory errors and learning errors. The distinction is as follows. When the animal runs through the maze over and over again, we expect to find that the number of errors decreases as practice continues. But if the animal runs all around the maze exploring the blind alleys, with or without system, as the case may be, then the Veeder counters will record errors which can hardly be considered a part of the urge to reach the food box. On the other hand, the animal does not tell us which is which so that, as far as the objective records are concerned, an error is an error. Our difficulty is in segregating analytically, if possible, those errors which can be called exploratory from those that constitute an intergral part of the desire to reach the food box. An observer noted that in one instance an animal found a bug in one of the blind alleys, whereupon he proceeded to make what seemed to be a thorough search for more bugs in the other blind alleys, including those that had been passed in the first part of the run through the maze. Evidently the errors so counted were in part to be attributed to a distracting motive which was spatially quite different from that of reaching the food box. As far as the animal was concerned, his entries into the blind alleys were not errors at all. They can be counted as such only from our point of view, and, in a sense, we are the ones who make these errors in calling them such.

If the errors that we have called exploratory were distributed in

all of the trials throughout the learning of the maze, it would probably be very difficult to separate them from the errors which contribute toward learning of the maze. But fortunately these exploratory errors seem to be most frequent in the first few runs through the maze and they occur relatively seldom in the later parts of the learning process. It is to be expected that the motive to investigate every corner of a maze will be more prominent in the first few runs than in the later part of the learning. On the assumption that exploratory errors occur mostly in the first few runs of the maze we should expect to find that they enter the relation of u and R in the form of a constant increment in the cumulative error count. The cumulative error curve, u against R, should appear as though it started with a certain u-intercept, a certain number of errors in one of the early trials which seems discontinuous with the rest of the errors. Some very good examples of this type can be found in Lashley's data. Because of these uncertainties we do not expect the hyperbola of equation 30 to fit the learning records for the first few trials, but the main course of learning for the extirpated as well as for the normal animals shows the expected form of cumulative error curve.

We have reproduced here the cumulative curves for several of the animals represented in Lashley's data. Only a few of the curves are shown here for the purpose of illustrating how the cumulative error curve may be used and interpreted. In Figure 2 we have the cumulative error record for animal No. 4 in Maze III. Beginning on trial 36, this animal made ten successive runs through the maze without error. We have fitted a hyperbola to these data and we find that the limit for this curve is 98 errors. The animal behaves in the first 45 trials in such a way that if he continued the same learning function that is shown in the data he would make 98 errors if his trials were continued indefinitely. This estimate ignores, of course, the fact that the animal must eventually die and that therefore the animal cannot actually make so many trials. It also ignores the fact that eventually such factors as fatigue or boredom or lack of attention would introduce errors in addition to those which are here regarded as learning errors.

In comparing the learning abilities of several animals, it is much better to compare the limits of their cumulative error curves than to compare merely the total errors that they make in some arbitrarily specified number of trials. For example, it is undoubtedly more

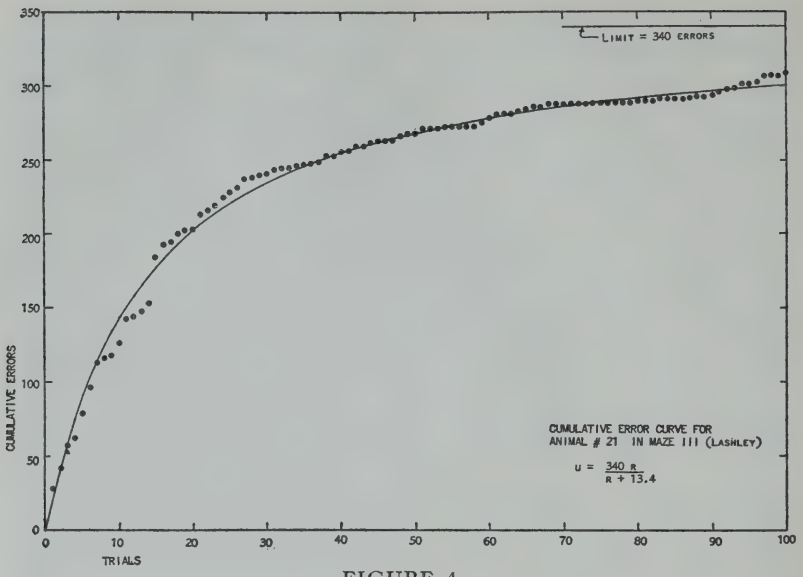

FIGURE 4

significant to compare the limit of 98 errors in Figure 2 with the limit of 340 errors in Figure 4 than to compare the total error count at, say, twenty trials. The latter would be 86 in Figure 2 and 204 in Figure 4. By making the comparison at the limit of the cumulative error curve we are using comparable stages in the two learning processes. It is obvious that twenty trials do not represent the same stage of learning in Figure 2 as they do in Figure 4.

It is possible to estimate the learning ability of an animal even though he has not yet completely mastered the maze. For example, suppose that the animal of Figure 4 has not mastered the maze after one hundred trials. It would still be possible to estimate the limit of the cumulative error curve for the one hundred trials. We need for this estimate a sufficient number of trials so that the cumulative curve shows some degree of curvature. Then the limit can be estimated with a fair degree of accuracy. The learning constant, k, of the animal is inversely proportional to the limit. In fact the limit is the numerical value of $\frac{\sqrt{m}}{k}$ in equation 30. If the trial num-

ber, R, is infinite in that equation, the last term vanishes and the limit, s, is then given by the simple relation

$$s = \frac{\sqrt{m}}{ak} \tag{31}$$

in which the constant a depends on the units of measurements so that it can be absorbed in the constant, m. This constant expresses the difficulty of the task and it is assumed to be constant throughout the learning of a maze.

In Figure 5 we have an illustration of another characteristic of the cumulative error curve. Note that the cumulative curve is continuous except near the origin. The record looks as though the 65 errors in the first two trials were discontinuous with the rest of the learning performance. We may refer to the u-intercept of the cumulative curve as the number of exploratory errors of the animal. They are evidently discontinuous with the remainder of his learning performance. It may be expected that the number of exploratory errors will be relatively small for those animals which have had

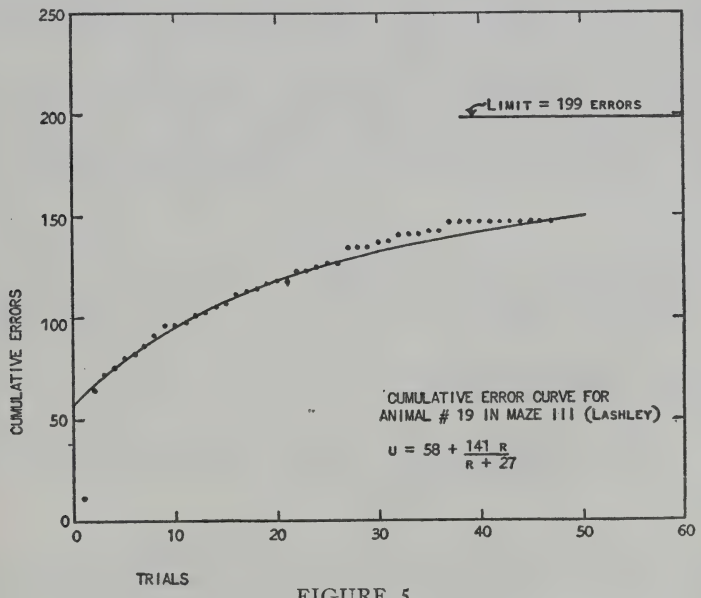

FIGURE 5

experience in maze learning. Exploratory errors in the first few trials are perhaps to be expected with animals that are entirely unaccustomed to the situation in which they must perform. It is an open question whether the learning ability of the animal should be judged by the learning errors alone or by the total number of errors, including the exploratory errors at the origin.

The object of this paper has been to transpose the writer's learning equation to the variables that are customary in maze learning, namely, errors and trials, and to describe the device of plotting the errors in cumulative form so as to reveal functional continuity in learning. We expected the cumulative error curve to be hyperbolic in form and this seems to be the form of most of the learning records in Lashley's data. It seems advisable to make comparisons of animals in terms of the limits of their cumulative error curves rather than in terms of the number of errors that they make in any arbitrarily specified number of trials. This assures that the comparisons are made at comparable stages in the learning process. It also seems feasible to estimate the number of exploratory errors which occur at the initial stages of learning since these are clearly not continuous with the remainder of the learning process. The cumulative error curve reveals continuities and other effects that are badly obscured in the usual form of error curve.

I wish to express my appreciation to Professor Lashley who has kindly made available his original maze-learning records of the animals described in his monograph. Our efforts have been to develop some quantitative methods that may be useful in the analysis of maze performance.

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LA FONCTION D'ERREUR DANS L'APPRENTISSAGE DU LABYRINTHE

(Résumé)

On représente ordinairement l'apprentissage d'un labyrinthe en représentant graphiquement le nombre d'erreurs à chaque épreuve contre le nombre ordinal des épreuves. Le résultat type est une courbe tombant

irrégulièrement vers l'apprentissage complet sans erreurs. L'auteur a constaté qu'on peut représenter mieux le progrès de l'apprentissage en représentant graphiquement les erreurs cumulatives contre le nombre ordinal des épreuves. On dérive une équation pour cette fonction d'erreurs cumulative laquelle s'accorde avec la fonction d'apprentissage employée antérieurement par l'auteur. On a analysé selon cette méthode les résultats de l'apprentissage chez les rats dans les expériences exploratrices de Lashley. Il parait donc qu'on peut séparer les erreurs d'extirpation et celles d'apprentissage plus facilement que par la forme usuelle de tabulation. La constante d'apprentissage de l'animal est le nombre total d'erreurs estimé dans un nombre infini d'épreuves. Si l'animal meurt avant la complétion de l'expérience, ou si l'expérience est interrompue, on peut d'ailleurs estimer sa constante d'apprentissage parce que la fonction d'erreur cumulative est hyperbolique. De cette manière on a pu comprendre dans l'analyse de l'expérience de Lashley les animaux qui sont morts. Au moyen de cette méthode on obtient les capacités des sujets à apprendre pour le même degré de l'apprentissage. C'est un processus supérieur à celui de comparer les sujets selon le nombre total des erreurs qu'ils ont faites pour divers degrés d'apprentissage. Ce dernier est le processus le plus usuel.

THURSTONE

DIE FUNKTION DER FEHLER IM LABYRINTH-LERNEN

(Referat)

Das Erlernen eines Labyrinths wird gewöhnlich durch die Aufzeichnung der Fehler pro Versuch gegenüber der Ordnungszahl der Versuche dargestellt. Das typische Ergebnis ist eine unregelmässige Kurve, die abfällt bis zum vollständigen Erlernen ohne Fehler. Der Autor hat gefunden, dass der Lernprozess besser durch die Aufzeichnung der kumulativen Fehler gegenüber der Ordnungszahl der Versuche dargestellt wird. Eine Formel für diese Funktion kumulativer Fehler wird abgeleitet, sie ist mit der Lernfunktion, die der Autor schon früher gebrauchte, vereinbar. Die Lernangaben über Ratten in Lashleys Entdeckungsexperimenten werden vermittelst dieser Methode analysiert. Es zeigt sich, dass Vertilgungsfehler so leichter von den Lernfehlern getrennt werden können als durch die gewöhnliche Form der Tabellarisation. Die Lernkonstante eines Tieres ist die geschätzte Gesamtzahl der Fehler in einer unendlichen Anzahl von Versuchen. Wenn das Tier vor Beendigung des Experiments zu Grunde geht, oder wenn das Experiment unterbrochen wird, kann seine Lernkonstante trotzdem geschätzt werden, da die Funktion der kumulativen Fehler hyperbolisch ist. In der Weise sind wir in der Lage in der Analyse der Lashley-Experimente auch die Tiere einzubeziehen, die nicht am Leben blieben. Durch diese Methode kann man die Lernfähigkeiten für denselben Lerngrad erhalten. Das ist ein besseres Verfahren, als der Vergleich der Versuchstiere nach der Gesamtzahl der Fehler, die sie bei verschiedenen Lerngraden aufwiesen. Die letztere ist aber das üblichere Verfahren.

THURSTONE

THE RATE OF ESTABLISHMENT OF A DISCRIMINATION*

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B. F. SKINNER[1]

I

One characteristic of the process of conditioning is its marked tendency toward generalization or spread. When a conditioned reflex is first established, all the properties of the newly conditioned stimulus are not essential in eliciting the response. Stimuli in which some of these properties are absent or replaced by others may be effective. But if only one property of the stimulus is experimentally correlated with the occurrence of the unconditioned or previously conditioned reflex upon which the conditioning is based, the response to a stimulus lacking this property is, of course, not reinforced. In such a case the organism eventually comes to respond to stimuli possessing the distinguishing property but not to respond to those lacking it. The process through which this is brought about is called discrimination.

In a discrimination we are concerned with at least two stimuli, S_1 and S_2, which possess properties in common but also differ from each other in some significant way. Two tones differing in pitch, two lights differing in intensity, or two groups of stimuli differing with respect to one or more of their members are typical examples. In establishing a discrimination both stimuli are first conditioned to elicit a common response (R). Subsequently the response is reinforced when elicited by S_1, but is not reinforced when elicited by S_2, so that the reflex S_1—R remains conditioned, while the reflex S_2—R declines in strength and eventually disappears. Technically the process of discrimination thus consists of the continued conditioning of one reflex and the concurrent extinction of another, where the two reflexes are related through properties of their stimuli (and through a common response). The phenomenon as observed is

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therefore only a special case of extinction, and no separate treatment would be required if the extinction and the continued conditioning could proceed independently. But this is not the case. The reinforcement of S_1—R affects the strength of S_2—R also, and there is a converse sharing of the extinction. The extent of the mutual interference is probably determined by the degree of community of the properties of the two stimuli.

It will be convenient to regard all such pairs of stimuli as constituting a series, in which the order is determined by the degree to which the members of a pair possess their properties in common. At one end of such a series the two stimuli in a pair are practically identical and the mutual interference is very great. In the limiting case we are in fact dealing with essentially only one stimulus, the response to which we sometimes reinforce and sometimes do not, and no discrimination is possible. At the other end of the series the two stimuli possess no significant property in common, and the two reflexes are virtually independent. Consequently the processes of conditioning and extinction may go on without interference, and in such a case the term "discrimination" is not used.

Most experiments involving discrimination are concerned with locating the upper limit of such a series within a given sensory field. That is to say, they attempt to evaluate the least differences between stimuli discriminable by the organism. They are directed toward the measurement of a sort of capacity, which may be either sensory or central. The process of discrimination enters into such an experiment only as part of its method, and no further inquiry is made into the process itself than is necessary in obtaining some convenient indication of its completion. In the present paper, however, we shall not be concerned with the capacity of the organism to form a discrimination. The stimuli to be used show relatively gross differences, and each pair would lie only midway along the series. Our main object is rather to follow the course of the development of a discrimination, and to determine as accurately as possible the properties of the process. In addition, we shall test the hypothesis that, if we are correct in regarding a discrimination as only a special case of extinction, the essential similarity of the two processes should be experimentally verifiable. A large part of each problem is a matter of definition or methodology, but the present paper is confined so far as possible to the report and analysis of experimental material.

The development of a discrimination is observed in any given

case as a change in the strength of the reflex undergoing extinction. It is assumed in the following discussion that the reinforced reflex remains maximally conditioned, or rather that any variation in its strength (which must be slight) may be ignored. The significant change appears in the state of the unreinforced reflex, which progressively declines in strength until it eventually disappears, when the discrimination is said to be complete. During the change the strength of the reflex may be measured by the magnitude of the response to a stimulus of a given constant value, as, for example, in Pavlov's method. The typical "discrimination-box" method uses the prepotency of the reinforced reflex over the unreinforced when the stimuli are presented simultaneously. In the present investigation, however, a third measure is used, namely, the rate at which the response is elicited under continuous stimulation. It has already been used in investigating the processes of conditioning and extinction (5, 6), and if the preceding account is essentially correct it should be adaptable to the present subject-matter.

II

The reflex chosen for the experiment and the conditions under which it is observed have been described elsewhere (4, 5, 6). Four identical sets of the apparatus are operated simultaneously. Each consists of a dark, well-ventilated, sound-proofed box, with inner dimensions of 45 cm. x 36.5 cm. x 23 cm., containing the following devices: (*a*) a compartment from which a rat may be released automatically after the box has been sealed, (*b*) a supply of drinking water, (*c*) a small inverted truncated cone, against one wall, with its upper rim 4.5 cm. from the floor, which serves as a food-tray, and (*d*) above the cone, 9.5 cm. from the floor, parallel to and 1 cm. from the wall, a horizontal bar, made of heavy wire, which may be pressed downward approximately 1.5 cm. against a tension of 10 grams. As the lever moves downward, a mercury switch directly behind the wall is closed. We are concerned with the response of the rat in pressing this lever, which we may define as any movement by the rat which results in the closing of the switch. The switch operates a food-magazine, which discharges a pellet of food of standard size into the tray, where it is accessible to the rat. The connection between the lever and the magazine may be broken at will by the experimenter. Throughout a series of experiments the

rats are fed exclusively with a commercial dog-biscuit. The pellets of food used in the magazine are of the same material.

The movement of the lever, whether or not it is followed by the discharge of food, is recorded in an adjoining room with apparatus previously described (4, 5). The method yields a kymograph record reading *time* on the abscissae and *number of responses to the lever* on the ordinates. Figures 1, 2, 4, 5, 6, 8, 10, 11, 13, 14, 15, 17 are reproduced from photostat copies of such records. In each case coordinates have been added and labeled. In the original records one hour is represented by 8.1 cm. on the time axis. The fairly great reduction in size has made it necessary to retouch the records in some few cases to obtain a clear reproduction. In order to avoid irregular contacts a modified form of the circuit-breaker previously described (3) has been used. The device acts to break the circuit from the lever for a short time following a response—in the present case for approximately two seconds, which does not include the time during which the lever is held down by the rat.

The only additional requirement for the investigation of a discrimination is an extra source of stimulating energy under the control of the experimenter. Two sources—one visual, the other auditory—have been used. The former is a small (3 c.p.) electric bulb placed against the wall considerably above and to the right of the lever. The other is the magnetic latch at the door of the release compartment, which is available, after the release, as a source of stimulation. It gives a clear double click.

This extra stimulating material is used in the following way. The experimenter controls the current to the light and the connection between the lever and magazine in such a way that the response to the lever-plus-light is always followed by the discharge of a pellet of food into the tray, while the response to the lever alone is never so reinforced. The animal eventually "learns to respond to the lever when the light is on but not to respond when the light is off." The stimulation arising from the lever plus that from the light is the S_1 of our paradigm; that from the lever alone is the S_2. The auditory stimulus is used in the same way, with the following exception. In the case of the light the extra stimulus, presented before the reinforced response, is still present when the response is made, but this is not true of the sound (since it is not continuous). No significant difference has appeared between the results in the two

cases, and the records are considered together in the following discussion.

In order to utilize the rate of elicitation as a measure of the strength of the extinguished reflex, it is necessary to control the temporal conditions of the experiment quite carefully. We shall proceed at once to a description of the present procedure. The reasons for adopting it will be explained after the results have been considered.

III

We have elsewhere described records of the extinction of the response to the lever after small amounts of reconditioning (6). In Figure 1 a series of four curves obtained during a two-hour period is reproduced. The response had been thoroughly extinguished on the preceding day. An extremely slight loss of extinction is evident at the beginning of the present record (at *A*), where the rat makes five scattered responses (unreinforced) during the first 8 minutes. The circuit to the magazine is then closed and the next response (at the vertical mark at *B*) is followed by the discharge of a pellet of food into the food-tray. The magazine is immediately disconnected. After the pellet is eaten the rat begins to press the lever at a high although diminishing rate, and during the next 25 minutes a typical curve for extinction is described. When a low rate of elicitation has been reached, the magazine is again connected (just before *C*) and the next response of the rat is followed by the discharge of a pellet of food. The magazine is then disconnected, and a second curve for

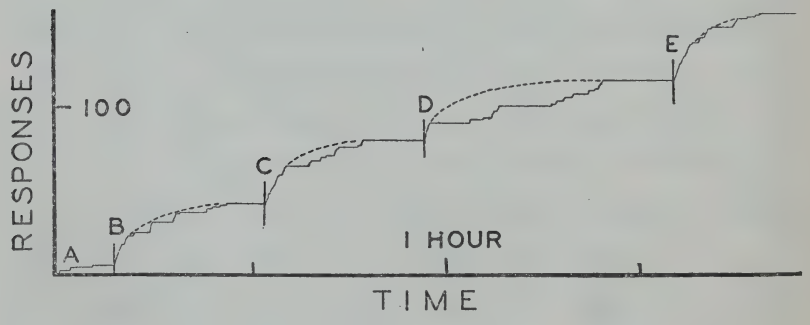

FIGURE 1

EXTINCTION CURVES FOLLOWING THE REINFORCEMENT OF SINGLE RESPONSES

extinction is obtained. The process is repeated at *D* and at *E*. A cyclic variation is apparent in all four curves, and in those at *C* and *D* the experimental curves are depressed considerably below the theoretical. These are typical effects, which have been discussed elsewhere (6).

In Figure 1 each reconditioning is effected when the curve for the previous extinction is practically complete. If the second reconditioning follows more closely upon the first, however, the remainder of the first curve for extinction appears to sum algebraically with the second. If the reconditioning is periodically repeated at some

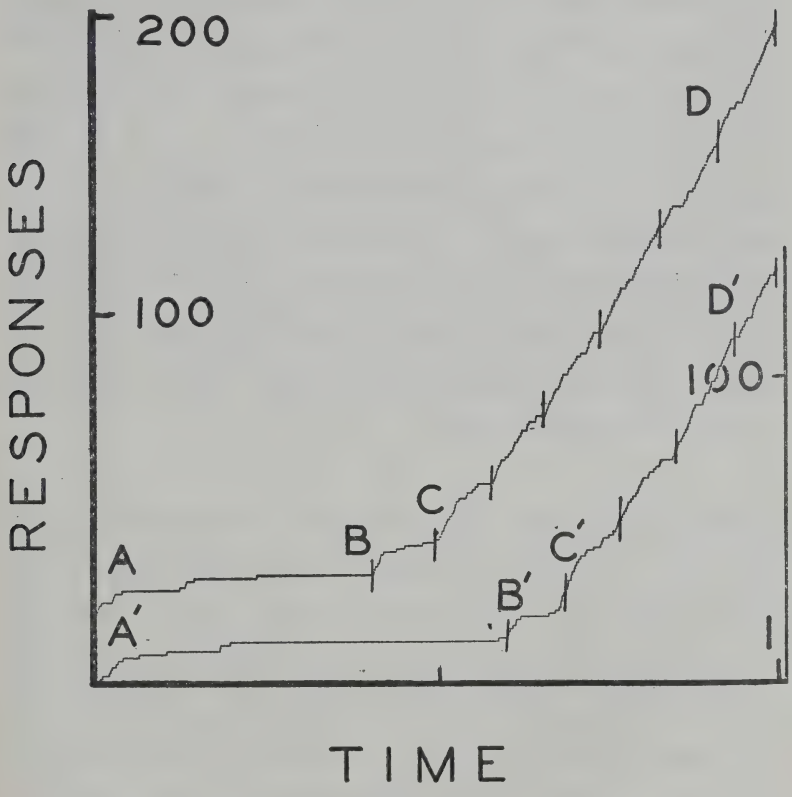

FIGURE 2

FUSION OF SUCCESSIVE EXTINCTION CURVES FOLLOWING THE REINFORCEMENT OF SINGLE RESPONSES (AT B, C, ETC.)

interval shorter than the average effectual length of the extinction curve for the amount of reconditioning employed, the successive curves continue to sum, until eventually a complete fusion takes place and the strength of the reflex remains at a constant value so long as the periodic reconditioning is maintained (for 24 experimental hours in the following experiments).

The development of a fused state is shown in Figure 2. The records are for two rats in which the response had been well extinguished on the preceding day. Some loss of extinction is shown in both cases (at *A, A'*), but the rate of elicitation quickly reaches a very low value, which is maintained for approximately 25 and 35 minutes respectively. At *B* and *B'* a schedule of periodic reconditioning is instituted, in which the response to the lever is followed by the delivery of a pellet of food every 5 minutes, and the intervening responses are allowed to go unreinforced. In the figure the reinforced responses have been indicated with vertical bars. The extinction curves at *B* and *B'* have the usual form, if we allow for the difficulty of approximating such a curve with as few as 10 or 12 discrete steps. The curves at *C* and *C'*, however, show much longer rising limbs, which, as we have said, apparently contain the responses remaining to the preceding curves. The third curves have still longer rising limbs, and, as the other reconditionings ensue, the character of each curve is finally lost and the records assume a constant slope. Thereafter the elicitation proceeds at a steady rate, and it is not possible to tell from the recorded behavior where the successive reinforcements occur.

IV

Against such a constant rate the process of discrimination is observed with the present method. The phenomenon is therefore important for our purposes and must be considered in detail. The following conditions of the general procedure may be noted. A special control of hunger is necessary. In experiments upon drive and conditioning reported elsewhere (3, 5) a fairly constant degree of hunger at the beginning of successive daily experiments was obtained with the simple method of feeding the animal once a day at the required hour. The effect of the routine was to develop a hunger cycle which reached its daily peak at the time of the beginning of the experiment. In a study of extinction, however, this condition cannot be fulfilled, since the animal does not receive any

considerable amount of food during the experimental period and must be fed its principal ration afterward. If such an experiment is repeated for several successive days, the peak of the cycle shifts from the time of the beginning to the time of the end of the experimental hour. Evidence for the shift may be obtained independently from a study of the spontaneous activity of the rat, using a method described elsewhere (7). In order to eliminate the shift, the present experiments were conducted on alternate days only, and on the intervening days the animals were fed in their living-cages at the beginning of the hour of the experiment. The intervening days are neglected in the following discussion, where such a phrase as "the preceding day" must always be understood to mean the preceding experimental day, with an intervening non-experimental day omitted.

In the first experiment to be considered four different intervals were interposed between successive reconditionings, namely, 3, 6, 9, and 12 minutes. These were assigned to four male rats, *P*7, *P*9, *P*8, and *P*10 respectively, aged 106 days at the beginning of the experiment. Since it is not possible to obtain the elicitation of a response immediately after the magazine has been turned on, the intervals cannot be exactly determined. In the present case the delay was added to the interval, and the resulting average intervals for the series were 3 minutes 6 seconds, 6 minutes 27 seconds, 9 minutes 47 seconds, and 12 minutes 59 seconds, respectively. In the other experiments to be described it was deducted from the succeeding interval, so that the average was exactly of the scheduled length.

Prior to this series of observations the response to the lever had been conditioned[2] and extinguished. Each animal was placed in its box at the same hour daily (9:00 A.M.) and approximately two minutes later was released into the main part of the box, where the lever was accessible. The first response to the lever was reinforced, and thereafter single responses were reinforcd according to schedule. All responses to the lever, whether reinforced or not, were recorded. A characteristic constant rate of elicitation was attained in each case. The animals were removed from the boxes at the end of one hour, and extra food was given to them in their living-cages for about two hours. No other food was given until the following (non-experimental) day, when, as we have said, a full ration was

[2]The records of the conditioning of this group corroborate the results reported in (5).

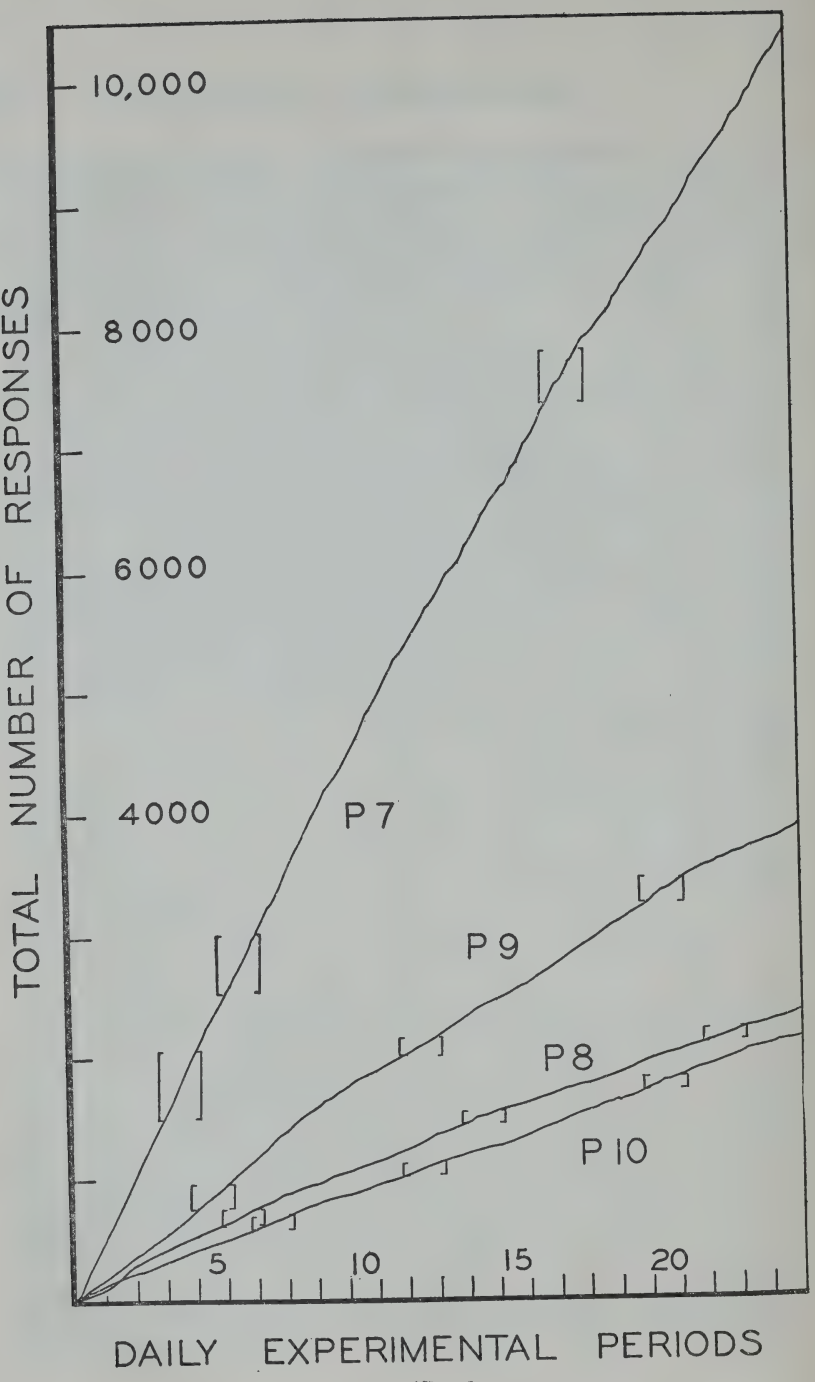

FIGURE 3

RESPONSES TO THE LEVER DURING 24 DAILY EXPERIMENTAL PERIODS OF ONE HOUR EACH

Reconditioning as follows: *P*7 every three minutes, *P*9 every six minutes, *P*8 every nine minutes, and *P*10 every twelve minutes

given at 9:00 A.M. This procedure was repeated for 24 experimental days.

The results are contained in four sets of 24 kymograph records, which describe the behavior of the rats with respect to the lever for a total of 96 experimental hours. The complete experiment is represented in Figure 3. The figure was constructed by plotting the total number of responses attained by the end of each day against the number of days. Between these separate points the approximate course of each daily record was drawn (free-hand). The details of the original records are lost in the unavoidable reduction in size. Typical records from each set, however, are reproduced in Figures 4 and 5, and their positions in Figure 3 are indicated with brackets in the latter figure, the order of occurrence corresponding in the two cases. Thus the curve for *P*7 in Figure 3 represents a series of 24 kymograph records pieced together to form a continuous graph, of which Figure 4 shows, reading from left to right, the fourth, sixth, and seventeenth members.

The most obvious conclusion to be drawn from Figure 3 is that the value of the assumed rate is a function of the interval between successive reconditionings. The shorter the interval, the steeper the slope of the graph. We shall return shortly to this relationship. It is also apparent that no very significant change in rate is to be observed at any given interval during the period of the experiment. So far as each daily record is concerned, there is, in fact, no consistent deviation from a straight line, although all records show local irregularities.[3] In the complete series, on the other hand, the rate of elicitation is not strictly constant, but shows a significant, though slight, decline. It is impossible at present to correlate so slight an effect with any specific condition of the experiment. The curves in Figure 3 cover a period of 47 days, a fairly large fraction of the active adult life of the organism, and there are many progressive changes to which an effect of this magnitude could be attributed. For any short section of the graph the curve may be regarded as essentially rectilinear.

V

Local deviations from a straight line peculiar to these records are of at least three clearly distinguishable orders. The first is unim-

[3]The curvature observed in some few instances (see *P*9, Figure 5) follows no consistent rule from day to day.

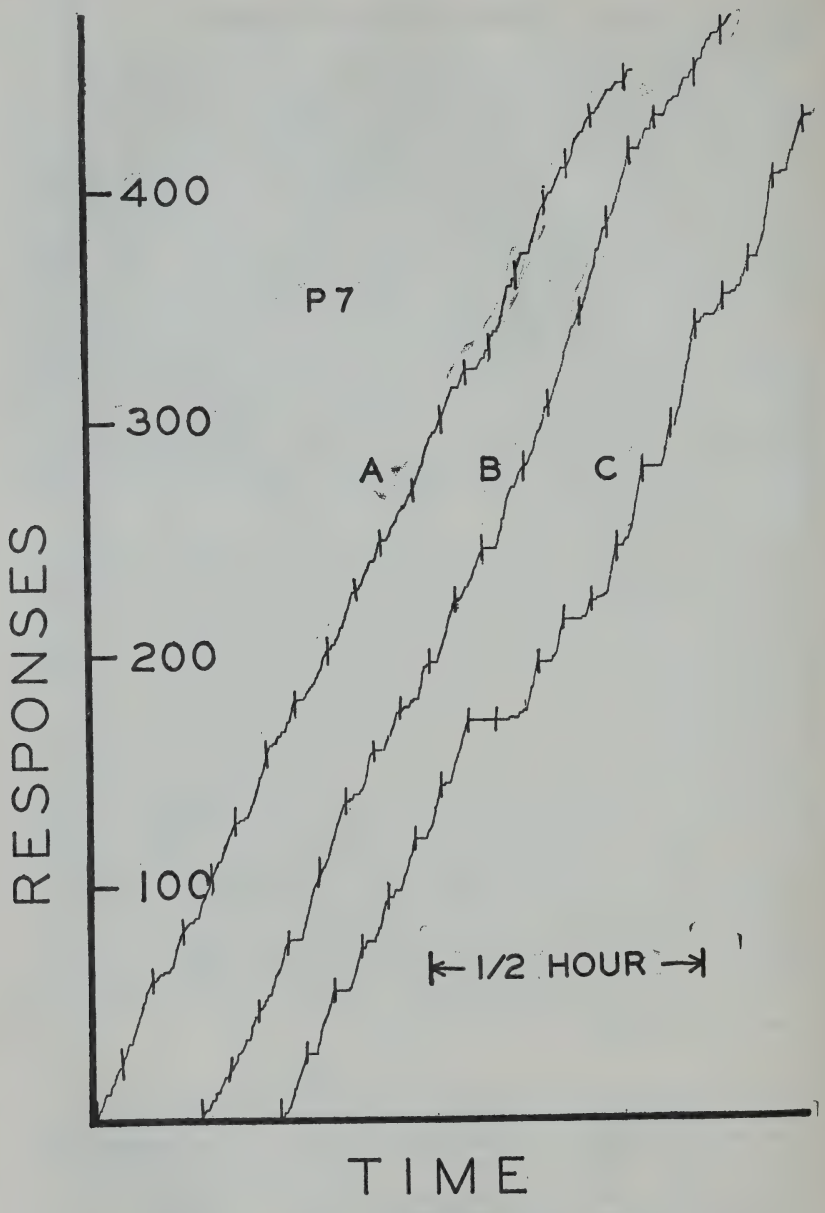

FIGURE 4

THREE DAILY RECORDS FROM THE SERIES FOR *P7* IN FIGURE 3

The positions of these records are indicated in Figure 3 by brackets.

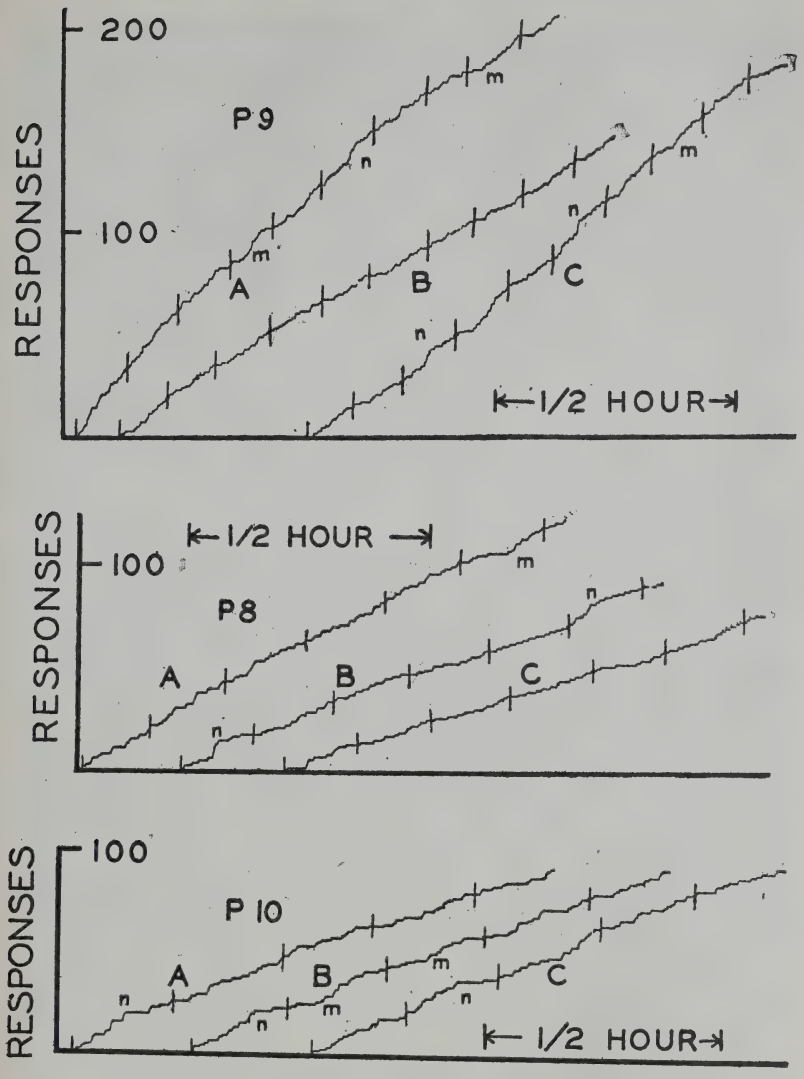

FIGURE 5

THREE DAILY RECORDS FROM THE SERIES FOR *P9*, *P8*, AND *P10* IN FIGURE 3
The positions of these records are indicated in Figure 3 by brackets.

portant but may be described. If we ignore the curvature of the graphs in Figure 3, we must still take account of a variation of the following sort. The rate of elicitation first shows a slight but continuous increase, which may extend over a period of several days and which may be obscured by the progressive decline in rate that occurs simultaneously. Then, within a day (or at most two days) the rate falls to a value that is minimal for the curve in question. From this point there is a slow recovery, which may persist for several days. When the rate has again reached a relatively high value, another rapid drop occurs, and the recovery is subsequently repeated. The range over which the rate varies is small, but it is sufficient to give the records a slight "scalloped" effect. This characteristic is exceptionally prominent in the curve for *P*9, Figure 3, but it can be detected in the other curves by strongly fore-shortening them. Examples have also been found in other (unpublished) experiments. The cause of the variation is unknown.

Deviations of a second order are much more striking and are definitely correlated with the special conditions of the experiment. They are found only at the higher rates of elicitation (induced with more frequent reconditioning) and are not present during the first days of the experiment. There is no reliable sign of the effect in Figure 4 *A*, for the fourth day of the series, although deviations of this type have clearly appeared by the sixth day (record *B*). They resemble those of the first order, with the important exception that the duration is of an entirely different order of magnitude. In Figure 4 *B* it may be observed that the average rate of elicitation gradually increases until the sixth reinforcement. It is then suddenly depressed. There is a subsequent steady recovery, leading to a second maximum at the fourth reinforcement from the end. Here another depression appears, the recovery from which is interrupted by the termination of the period. The record is thus divided into three segments, each of positive acceleration, which meet at points showing definite discontinuity. The effect has become more clearly marked (although in this case somewhat more irregular) by the seventeenth day, record *C*.

A careful examination of these variations of the second order would take us too far afield, but we may note the following characteristics. First, the average basic rate of elicitation is unchanged. The three records in Figure 4, for example, are approximately parallel. We may therefore conclude that, secondly, the effect has

two discrete phases, one marked by an increase above the basic rate, and the other by a depression below it, which combine to give the basic rate as a resultant. The two phases may be observed in Figure 4 and in the figure about to be described (Figure 6). Thirdly, when a restriction is placed upon one phase, there is a corresponding adjustment of the other. Thus in Figure 4 the basic rate is already so high that any increase is probably faced with a practical limit.[4] Accordingly, the depressed phase has no extensive compensation to make and is of fairly short duration. The two records in Figure 6, which may be compared with Figure 4, were obtained on successive days at the end of a long series of observations upon another rat (*K*12) where the interval of periodic reconditioning was five minutes. The basic rate (given approximately by a straight line drawn through the end points of each record) is relatively low. A very large increase is therefore possible and is actually observed—notably in the initial segments of the two curves. The periods of depressed rate are correspondingly more extensive. Figure 6 is thus a much better example of the second-order effect, since it demonstrates a fuller development of the augmented phase. It also shows clearly that the fundamental deviation is the increase in rate, and that the depressed phase enters only as a compensating factor.

The nature of variations of the second order is not wholly clear. Recalling our ideal series of all pairs of stimuli, we may regard the present procedure as one in which the two stimuli in a discrimination are identical. The conditions are therefore very similar to those in which there is an actual *but subliminal* difference between the stimuli. In the latter case a phenomenon of "breakdown" has been observed (*cf.* Pavlov, 1), which is characterized by sudden and usually violent changes in the strength of the reflex. The present effect may be one form (possibly only an incipient form) of breakdown. In any event the reality of the phenomenon can hardly be questioned. The instances shown in the accompanying records (and in very many others) cannot possibly be due to any chance distribution of various rates of elicitation.

Deviations of a third order appear as depressions in the rate of elication after the periodic reconditioning of the reflex. They are followed typically by compensatory increases, so that the total rate (as we have already seen) is unchanged. With a relatively short

[4]The apparatus, however, is capable of handling a rate at least 50 per cent above the highest here observed.

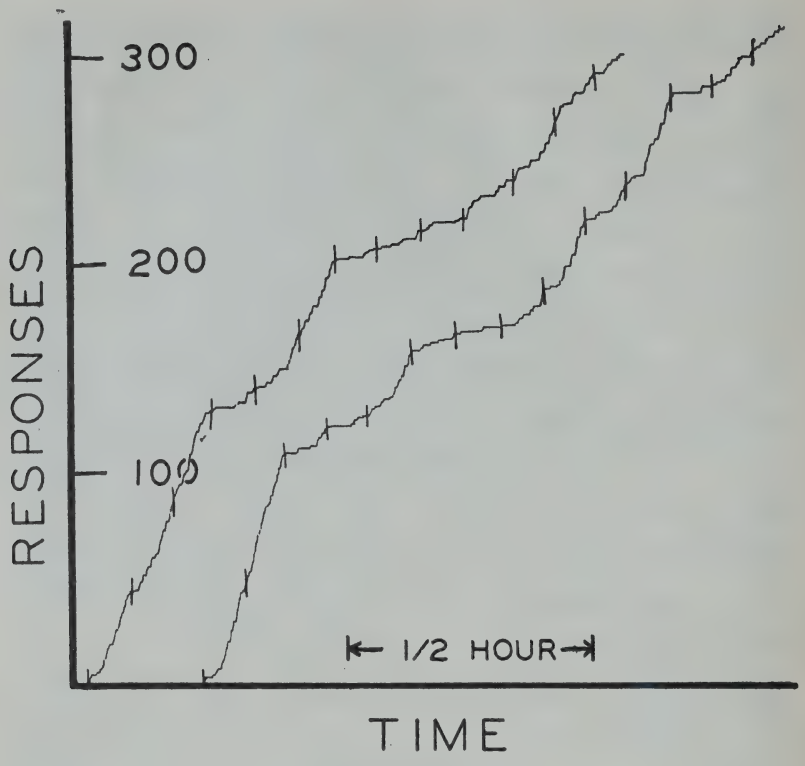

FIGURE 6

RECORDS FOR TWO SUCCESSIVE DAYS UNDER PERIODIC RECONDITIONING AT INTERVALS OF FIVE MINUTES, SHOWING UNUSUAL DEVELOPMENT OF DEVIATIONS OF THE SECOND ORDER

period of reconditioning the depressions begin to appear within a few days. They are already present to some extent in Figure 4, record *A*, and have become quite definite in record *B*. They give a step-like character to record *C*, on the seventeenth day of the series. The same effect is apparent, though to a lesser degree, in the records obtained with reconditioning at longer intervals (see Figure 5). Here their development is considerably retarded.

Such an effect would ordinarily be described as a conditioned inhibition. It is not clear, however, that the events fit any of the usual formulae for conditioning. The phenomenon may be regarded as reflex inhibition if we define the latter simply as a decrease in the

strength of a reflex correlated with the presentation of a second (extraneous) stimulus. Our observations, however, are limited to the following. Because of the character of the experimental procedure the relatively constant stimulation in the experimental box is supplemented periodically by a group of stimuli arising from the discharge of food from the magazine and from the ingestion of food by the rat. The periods of depressed rate that appear in our records are definitely correlated with the occurrence of this extraneous stimulation. Accordingly, by definition, the stimulation may be said to set up an "inhibitory effect." We may regard it, furthermore, as a *conditioned* inhibition, if that term has no more specific meaning than that the relation between the extraneous stimulus and the inhibitory effect is dependent upon the experimental history of the organism. In Appendix I we shall consider the possibility that these third-order deviations constitute a temporal discrimination, but for the present we shall not go beyond this simple description of the data.

We may note in addition, however, that there is a close relation between deviations of the second and third orders. Both appear on about the same day of the experiment and the beginning of the depressed phase of the second-order effect almost invariably coincides with a depression of the third order. Furthermore, since the extent of the deviations of the third order is in general a function of the rate, it varies with the rate during changes of the second order.

There are also deviations of a fourth order to be considered, but they are not peculiar to records obtained with this procedure. In the simplest case they are the result of the generally observed tendency of responses to occur in groups rather than in an equally spaced series—apparently an effect of threshold. In the more severe form of the deviation a period of no responding may become considerably extended—a matter either of threshold or of prepotency. A period of increased rate then follows, by virtue of which the rat "recovers" from the delay (3, 6). As a converse sort of deviation, which has so far been observed only in experiments involving extinction, the period of rapid responding may occur first, followed in turn by a compensating period of reduced rate (6). A few examples of the two effects have been indicated in Figure 5. Those marked *m* show an initial depression in rate followed by an increase; those marked *n* show the reverse. Many examples may be found in the other figures.

These four orders are clearly definable in terms of their observed

properties. Taken together, they account for all the deviations from a straight line found in the present records. The only possibility of confusion is between the third and fourth orders, which are of approximately the same magnitude: if a devaition of the fourth order should occur immediately after the reinforcement of a response, it would be indistinguishable from one of the third. For this reason it is difficult to detect the first appearance of the third-order effect, although the difficulty may be eliminated with a statistical treatment. In dealing with the process of discrimination we shall, as we have said, find it convenient to regard the graphs in Figure 3 and the daily curves composing them as essentially rectilinear. In this respect the deviations that we have examined raise no special problem. All four types show compensatory readjustments, and in no case is there a permanent deflection of the curve. Moreover, in the experiments upon discrimination we shall use a fairly moderate rate of elicitation, induced with a five-minute period of reconditioning, where deviations of the second and third orders are not present to any significant extent. The change observed during the establishment of a discrimination is also fortunately of such an order of magnitude that confusion with any one of these types is very unlikely.

VI

Having examined the extent to which our experimental curves are to be regarded as rectilinear, we may return to the relation between a given rate of elicitation and the period of reconditioning at which it is assumed by the rat. That the interval and the slope vary inversely is apparent in Figure 3, but the relationship is not wholly reliable because of the probable presence of individual differences. These may be allowed for either by using a large number of animals at each slope or by rotating a small number over a series of slopes. The former method has not been tried. An experiment with the latter yielded the following result.

Four intervals were chosen to cover the greater part of the change in slope in Figure 3, viz., 3, 5, 7, and 9 minutes. Four rats (*P*3, *P*4, *P*5, and *P*6), of the same strain and approximately the same age as in the previous experiment, were tested at all four intervals according to the schedule in Table 1.[5] Each rat remained at each inter-

[5]This group had been conditioned 50 days before. Meanwhile, on alternate days, several aspects of the response had been studied. Most of the experiments required periodic reconditioning at 15-minute intervals for periods of 1 hour.

TABLE 1
INTERVALS OF RECONDITIONING

Day	*P*3	*P*4	*P*5	*P*6
(0)	(5)	(5)	(5)	(5)
1	3	5	7	9
2	3	5	7	9
3	5	7	9	3
4	5	7	9	3
5	7	9	3	5
6	7	9	3	5
7	9	3	5	7
8	9	3	5	7
9	3	5	7	9
10	3	5	7	9

TABLE 2
OBSERVED AVERAGE SLOPES

Interval	*P*3	*P*4	*P*5	*P*6	Mean slope
3 min.	53.2°	40.5°	62.9°	52.5°	52.3°
5 min.	47.5°	40.3°	59.0°	43.5°	47.6°
7 min.	44.0°	34.0°	54.8°	35.0°	41.9°
9 min.	36.0°	31.5°	44.5°	29.2°	35.3°
Average	45.2°	36.6°	55.3°	40.1°	

val two days and returned on its last two days to the interval at which it began. The slopes of the 40 records so obtained were measured with a protractor, using the beginning and end of each curve as the only significant points. The average slope for each rat at each interval is given in Table 2. Each entry is the average of two observations, except for the interval at which each rat began (see Table 1), where four observations were taken. That there are large individual differences is shown by the average slopes for the series regardless of length of interval, which range from 36.6° to 55.3°. The mean slope at each interval is free from the effect of this individual varation and is the result we desire.[6]

Before calculating the standard deviation of the mean, it is necessary to correct for individual differences. This has been done in

[6]The possible effect upon the means of the extra weight of the four-observation averages may for our present purposes be ignored.

the following way. A plot of the values in Table 2 justifies the tentative assumption that individual variation from the mean is absolute and may be expressed as a constant number of degrees, independent of the slope, within the range here observed. On this assumption the values for each rat were modified by adding or subtracting a characteristic number, which was chosen so that the sum of the squares of the deviations of the resulting values from the mean was a minimum (obtained graphically). The number chosen for each rat is given in the last line in Table 3. The corrected values, their means, the tangents, and the standard deviations of the means are also given in that table. The result is shown graphically in Figure 7, where the tangents of the means in Table 3 are plotted

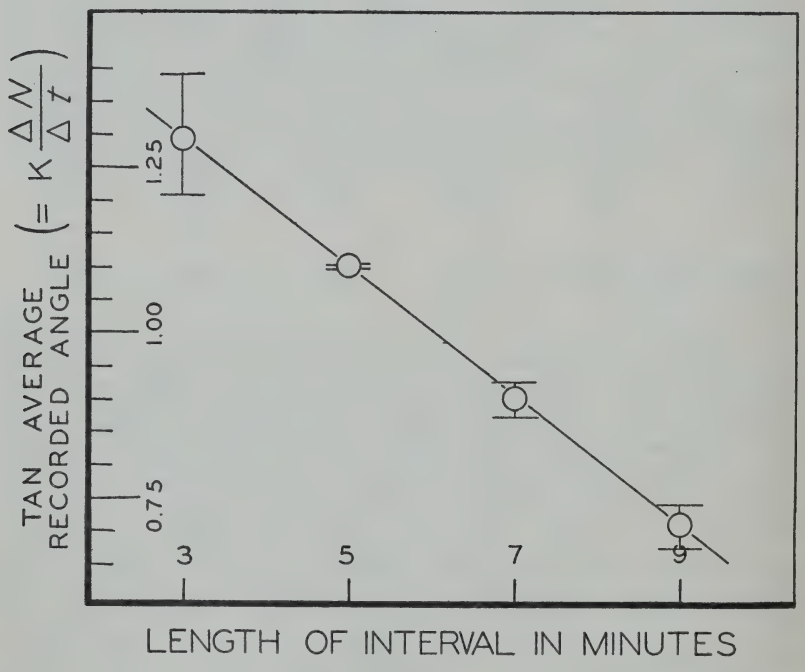

FIGURE 7

RELATION BETWEEN RATE OF ELICITATION (GIVEN BY THE TANGENT OF THE ANGLE MADE BY THE RECORD WITH THE HORIZONTAL) AND THE LENGTH OF THE INTERVAL OF RECONDITIONING

Horizontal bars mark off twice the standard deviations of the points.

TABLE 3

AVERAGE SLOPES CORRECTED FOR INDIVIDUAL DIFFERENCES

Interval	*P*3	*P*4	*P*5	*P*6	Mean slope	Tan mean slope	S.D.$_m$
3 min.	52.3°	48.2°	51.8°	57.0°	52.3°	1.29	±1.81°
5 min.	46.6°	48.0°	47.9°	48.0°	47.6°	1.10	±0.04°
7 min.	43.1°	41.7°	43.7°	39.5°	42.0°	0.90	±0.93°
9 min.	35.1°	39.2°	33.4°	33.7°	35.4°	0.71	±1.34°
Correction	—0.9°	+7.7°	—11.1°	+4.5°			

against the length of interval.[7] The horizontal lines mark off twice the standard deviation of each point. A very good approximation to a straight line is apparent.

This relationship is doubtless to some extent fortuitous, since it is impossible in so short a series of observations to correct for the effect of "carry-over" from one experimental day to the next. If the rat has been responding to the lever at some constant rate, that rate will be assumed at the beginning of a new experimental period, even though the frequency of reconditioning in the new period is going to justify either a lower or a higher rate. Adjustment to the new frequency is quickly made, but the average for the full period necessarily shows the effect of the anomalous initial rate. Accordingly, since the intervals of 5, 7, and 9 minutes in the present schedule follow *shorter* intervals in each case, we should expect the values of the slopes observed upon the first days at these intervals to be to some extent too high. The three-minute intervals, on the other hand, follow longer intervals in every case, and the observed slopes are therefore probably too low. An actual carry-over is evident in most of the records as an initial positive or negative acceleration. This difficulty is inherent in the method and cannot be dealt with adequately until the extent and nature of the carry-over are better known. Some allowance could have been made by estimating the slope eventually attained at each interval, rather than respecting only the end points of each record, but this would have introduced an element of interpretation, which we have desired to avoid. The result given in Figure 7 is not intended to be independent of the particular

[7] We are interested in the tangent of the angle rather than in the angle itself, since the significant datum is the rate of elicitation.

conditions of the experiment. A relationship between slope and interval, free of the effect of any significant individual difference, is demonstrated, but its precise nature is not shown.

VII

In saying that the constant rate observed under periodic reconditioning is the result of the fusion of many separate extinction curves, we intend only to describe the behavior of the rat as reported, for example, in Figure 2. But even at this level the description is inaccurate, for a mere algebraic summation will not wholly account for the resulting curve. In every case so far examined the slope eventually assumed is less than the initial slope of the extinction curve, and it may be very much so where the frequency of reconditioning is relatively low (cf. Figure 5). In addition to the summation, therefore, we must recognize a factor operating to depress the rate immediately after reconditioning. Since it is originally not present, it may be supposed to be a "conditioned inhibition" similar to the deviations of the third order already described. It is possible that a single factor is involved in both cases, of which the third-order effect proper is only an extreme development. At the lower slopes the effect of the inhibition is to produce a surprisingly accurate leveling of the rate over an extensive series of experimental periods. Thus the records in Figure 5 give in general no reliable indication of the time at which reconditioning occurs. As the experiment progresses the local effect of each reconditioning is thus lost, and the only result to be observed at all is the continued maintenance of the steady rate. It is as if the repeated reconditioning had established a large reserve, which became the chief factor in determining the rate, and against which a single reinforcement could have little effect. We are still close, however, to a point at which the specific effect of the reconditioning may become apparent. Thus if alternate intervals of two and eight minutes are substituted for the usual five and five, the same total slope is maintained, but a local effect of the reconditioning is evident in the wave-like character of the record (Figure 8). The combined depressive effect of each pair is clearly shown.

VIII

The phenomena observed under periodic reconditioning obviously demand consideration in their own right. Only as much has been

given here as is necessary to an understanding of the experiments that follow. The principal fact with which we are concerned is that under an experimental procedure of alternate reconditioning and extinction a reflex assumes a constant strength, and that at a given frequency of reconditioning this strength is practically invariable. The relation of the procedure of periodic reconditioning to that of discrimination should be obvious. The former may be converted

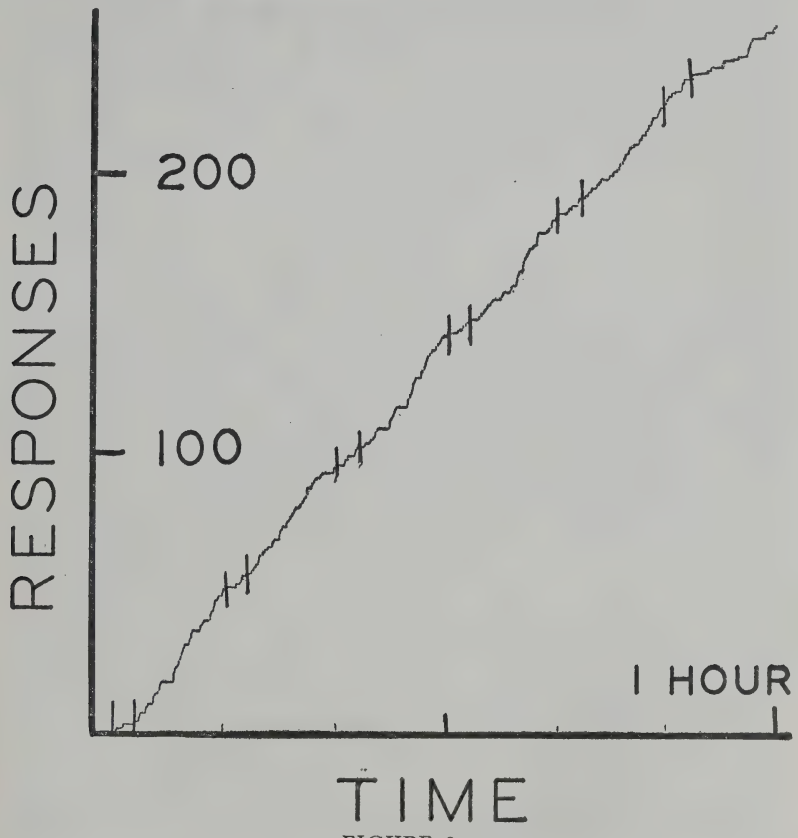

FIGURE 8

WAVE-LIKE RECORD OBTAINED BY GROUPING SUCCESSIVE RECONDITIONINGS, GIVING INTERVALS OF TWO AND EIGHT RATHER THAN FIVE AND FIVE MINUTES

The local depressive effect of the reconditioning is additive.

into the latter simply by introducing extra stimulating material whenever the response is either reinforced or extinguished. There are then two stimuli that elicit the response (both of which are, strictly speaking, *groups* of stimuli), which differ only with respect to the added member. We shall consider here only the case in which the addition is made in such a way that the response to the group containing the added material is reinforced and the response to the group lacking it is not. It will be readily understood that the change to appear experimentally during the development of the discrimination is a progressive lowering of the slope of the graph established under the procedure of periodic reconditioning.

Four male rats of the same strain, *P*11, *P*12, *P*13, *P*14, were conditioned to respond to the lever at the age of 100 days. Three days later they were transferred directly to periodic reconditioning, without any intervening extinction,[8] at an interval of five minutes. This interval had given the lowest standard deviation in the experiment in Figure 7. It also yields (with this strain of rats) a rate moderate enough to permit significant changes in both directions and practically free of deviations of the second and third orders. The experiments were conducted for periods of one hour on alternate days, and the other details of the procedure outlined above were followed. Three daily records of periodic reconditioning were obtained, during which characteristic constant slopes were well stabilized. The procedure was then changed to include either the light or the sound (sound with *P*11 and *P*12, light with *P*13 and *P*14).

The discriminative procedure for a single day was as follows. As the animal was released, both the light and the magazine had been connected, and the first response to the lever, in the presence of the light, was reinforced with a pellet of food. Both light and magazine were then turned off, and the responses during the next five minutes were unreinforced. At the end of that time the light and magazine were turned on, care being taken that this was not synchronous with a response. When a response had been made (and reinforced), the light and magazine were again turned off, and this was repeated for a total of twelve reinforcements. At the end of the last interval no response was reinforced, and the animal was removed from the

[8]Differing in this respect from *P*7, *P*8, *P*9, and *P*10. The curves for the first day show an algebraic summation of the typical extinction curve with the curve of positive acceleration obtained at the beginning of periodic reconditioning after previous extinction (Figure 2).

box. The sound was used in the same way, with the exception already noted. The procedure was repeated for ten days.

The delay in obtaining the pellet was not added to the interval. This not only simplifies the treatment of the data but is a necessary part of the procedure in a discrimination, since the first significant change in the behavior of the rat is the shortening of the delay. The response to the lever-plus-light (or sound) immediately increases in strength from the resultant value given by the constant slope to

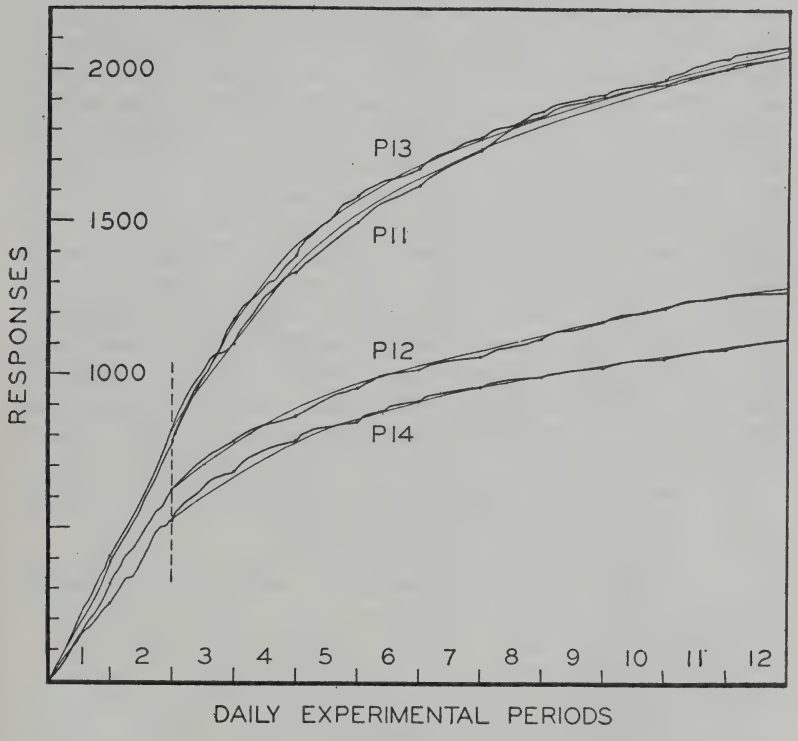

FIGURE 9

CHANGE IN THE SLOPE ASSUMED UNDER PERIODIC RECONDITIONING AFTER THE INTRODUCTION (AT THE VERTICAL BROKEN LINE) OF A DIFFERENTIATING STIMULUS AT EACH RECONDITIONING

Heavier lines are experimental. Lighter lines are for the equation $N = K \log t + C + ct$ where N = number of responses at time t and K, C, and c are constants having the magnitudes given in the text.

an approximately maximal value. As we have noted, we are not concerned with this change in the strength of the reinforced reflex,[9] but the change in the length of the delay must not be allowed to affect the interval. Otherwise the correlated change in the total slope would appear as an artifact.

The result of the experiment is given in Figure 9, where the records for each rat for two days prior to and for ten days following the change in technique have been pieced together to form continuous graphs (the heavier lines in the figure). Since the detail is again lost in the reduction, the first eight records after the beginning of the discrimination have been reproduced from two typical sets, in Figures 10 and 11. Figure 10 is for a rat showing a relatively high basic rate under periodic reconditioning; Figure 11, for one showing a lower rate. In Figure 10 a sound was used in establishing the discrimination; in Figure 11, a light. There is no correlation between a greater slope and the use of a sound.

The first two days in Figure 9 are given as controls. They establish the slopes assumed under periodic reconditioning immediately prior to the discrimination.[10] In view of the result given in Figure 3, we may assume that no significant change in these slopes will be observed so long as the experimental procedure is unmodified. It is evident, however, that, as soon as the technique is converted into that of discrimination by the introduction of a differentiating stimulus, the slope of each curve begins to fall off. The strength of the reflex in response to lever-plus-light remains maximal, but the reflex in response to the lever alone gradually decreases in strength, and a very low value is eventually reached.

The change is not rapid enough to be very clearly revealed at the end of a single hour. Nevertheless, the records for the first day evidently depart from the usual straight line and assume a characteristic curvature, convex upward, which is also assumed on succeeding days. The course may be at times severely distorted by deviations of the fourth order. In particular there is a tendency toward overshooting—a period of very active responding followed by a compensatory decrease in the rate of elicitation. Seven ex-

[9]The rate of the change may be estimated from the fact that an average basic value for the delay varying between two and ten seconds is reached after about ten reinforcements.

[10]Some individual variation is apparent, as we should expect from the experiment described in Section VI.

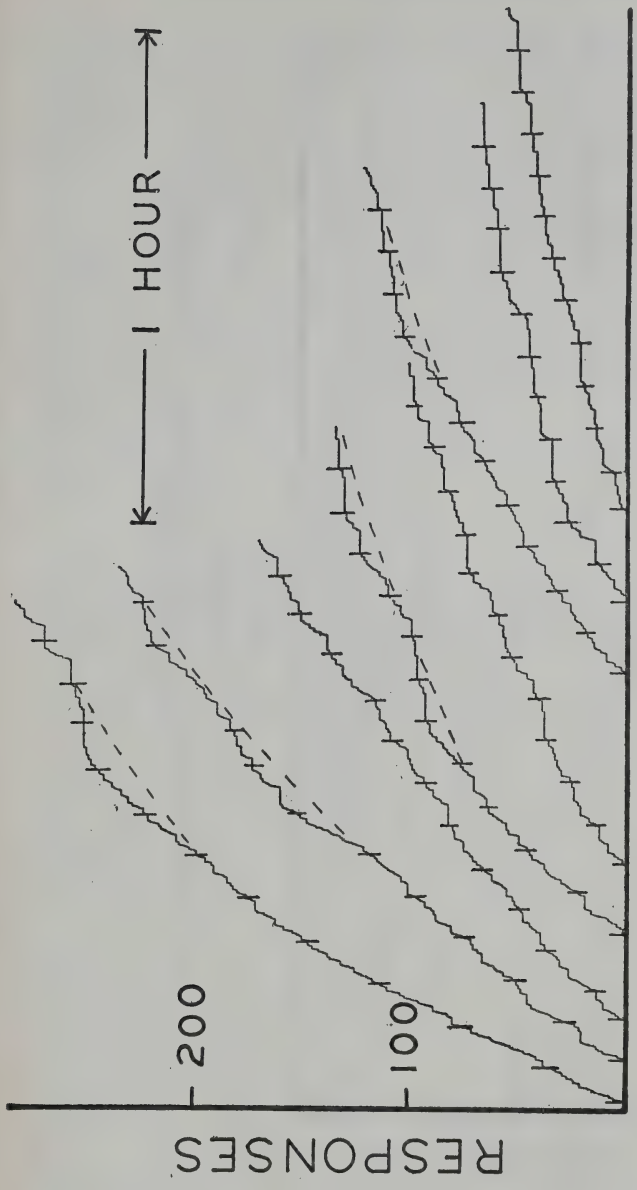

FIGURE 10

First Eight Daily Records for *P*11 in the Series Beginning at the Vertical Broken Line in Figure 9

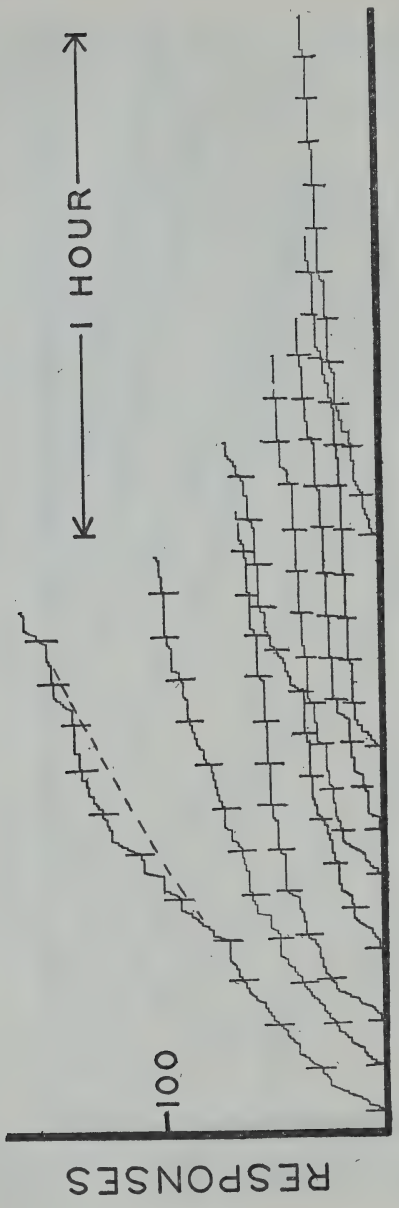

FIGURE 11

First Eight Records for *P*14 in the Series Beginning at the Vertical Broken Line in Figure 9

amples of the latter effect have been indicated in Figures 10 and 11, where the probable course of each curve in the absence of the effect has been traced with broken lines. Other deviations are less extensive, and in general the course of each record may be easily inferred. With an occasional exception the daily slope progressively declines during the ten days of the experiment.

No attempt will be made to describe the change taking place during a single hour. It is comparatively slight, and the records are subject to too many incidental variations to make a quantitative description either convincing or useful for our present purposes. The greater change during the full course of the discrimination is, however, more significant. We may deal with this by respecting only the end points of each daily record. It is not difficult to describe these with an empirical equation. In Figure 9 theoretical curves (the lighter lines) have been drawn through the experimental data. They are for the equation

$$N = K \log t + C + ct$$

where N = number of responses to the lever and t is time in hours from the beginning of the use of the discriminative technique. K, C, and c are constants having the following values:

	*P*11	*P*12	*P*13	*P*14	Av.
K	825	295	765	375	565
C	265	130	370	105	218
c	15	25	15	10	16

We are interested, to be exact, in the first derivative of this equation, since our fundamental datum is the change in rate. For obvious reasons we are forced to record the integral form of the curve, and in order to remain as close as possible to our observations this form is reproduced here. Little would be gained in publishing at the same time corresponding figures obtained through graphical differentiation. To avoid confusion, therefore, we shall leave the equation also in the integral form. It should be noted that the coordinates in Figure 9 do not begin at zero so far as the process of discrimination is concerned.

The validity of this description is restricted by the following considerations. First, we may repeat that the fit is to the end points of the daily records only. We could not expect to fit the body of each curve with so simple an equation. The time represented by t is not continuous, but a succession of discrete periods, and

the curve for discrimination, interrupted at the close of one hour, does not continue unchanged at the resumption of the experiment 47 hours later.[11] Lacking a quantitative treatment of the daily change, we must resort to some such treatment as the present, where the records are pieced together to form continuous graphs. We are thereby, of course, simply taking the height of the daily curve at an arbitrary point as a convenient measure of the behavior for the day.

The practice introduces a probable source of error: when a given daily experiment is brought to its close, the curve may be at the moment in the course of a deviation. As in the experiment described above in Section VI, we could allow for this by estimating from the body of the curve the most probable end point. We wish, however, to introduce no such element of interpretation into the present report. And in accepting the end point as recorded we are possibly justified by the probability of a compensating adjustment on the following day. The most serious deviations in the present case are those of "overshooting." As a result, the end points of the records for the first days (where the overshooting is most prominent) lie, if not upon, above the theoretical curves.

Another possibility of error arises because the daily rate certainly depends in some way upon the hunger of the rat, although this point has not been checked experimentally for exactly these conditions. In the present study we were not prepared to include hunger as an independent variable, and it was therefore necessary to maintain it as constant as possible. The results obtained with periodic reconditioning (Figure 3) are an adequate check against any significant variation from day to day. Nevertheless, even with the most careful feeding, occasional temporary disturbances in the condition of an animal are certain to appear. The sixth record for $P11$ in Figure 10, for example, is obviously much too high, and it is reasonable to attribute the abnormality to a condition of increased hunger. It is not clear whether the extra responses in the record are borrowed from later records in the series, or whether the total curve is displaced bodily from the anomalous day onward. The curve for $P11$ in Figure 9 has been fitted, however, as if the increase on the sixth day were compensated for during the two days immediately following. In this case the treatment seems to be justified by the records, but

[11]The increase in the strength of the reflex during an interval of this sort has been noted elsewhere [cf. the discussion of "loss of extinction" in (6)].

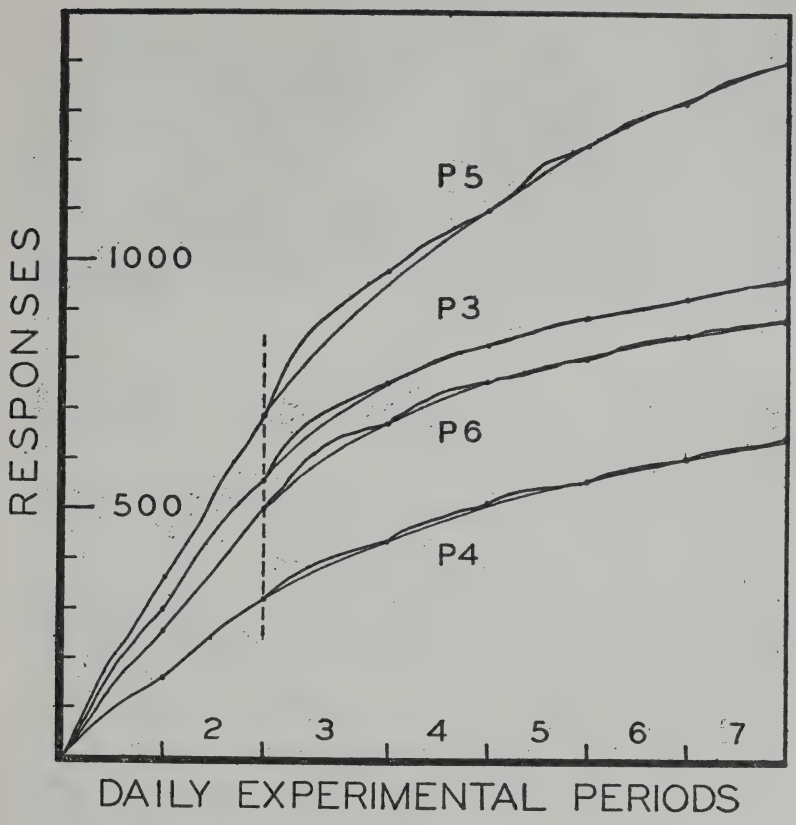

FIGURE 12

CHANGE IN THE SLOPE ASSUMED UNDER PERIODIC RECONDITIONING WHEN THE RECONDITIONING IS DISCONTINUED (AT THE VERTICAL BROKEN LINE)

Heavier lines are experimental. Lighter lines are for the same equation as in Figure 9.

the general interpretation of the effect of hunger obviously requires further experimental work.[12]

Within these limits, the curves we have obtained represent the development of a discrimination, and the process may therefore be described tentatively with an equation that is logarithmic and contains three constants. We have elsewhere reported that the curve

[12]The term "hunger" is used here in accordance with a previous discussion (3).

typically obtained for the original extinction of such a reflex is also logarithmic (6). This is significant. It indicates that the two processes are, as we have said, related; and that in a discrimination we are in fact dealing with the continued reinforcement of one reflex (which does not affect the slope observed under periodic reconditioning) and the concurrent extinction of another (which produces a progressive decline). However, we have not examined extinction under the present circumstances, that is to say, after prolonged periodic reconditioning. We must turn to this question before extending our comparison.

IX

The properties of extinction following prolonged periodic reconditioning have been studied with the group of four rats used in the experiment represented in Figure 7 (*P*3, *P*4, *P*5, and *P*6). Following the rotation of four intervals as described the rats were given three days with alternate intervals of two and eight minutes (records of the type shown in Figure 9 were obtained) and two days with intervals of five minutes. The response to the lever was then extinguished for five days. The rats were placed in their respective boxes on these days in the usual way, but the periodic reconditioning was omitted, and none of the responses to the lever was reinforced.

The results are given in Figure 12, which was constructed in the same way as Figures 3 and 9. The first two days give the control slopes under periodic reconditioning at an interval of five minutes.[13] The individual variation follows the order already given for these rats in the fifth line in Table 2, that is, $P5>P3>P6>P4$. The extinction extends from the third to the seventh day. The first four records during extinction are reproduced from two typical sets (*P*3 and *P*4), in Figures 13 and 14 respectively. By comparing Figure 12 with Figure 9 (and noting the difference in scale) it will be seen that we are here concerned with a more rapid change. Although the records for the first day of discrimination showed some curvature, it was slight. In extinction, on the other hand, there is an extensive change within a single day.[14] Subsequently the two

[13]The record for the second day for *P*6 was destroyed through a difficulty with the apparatus. The slope given in Figure 12 was estimated from the previous performance of the rat.

[14]In order to show the character of the change more clearly the first period has been extended to 1¼ hours. In fitting the curves for the series

FIGURE 13

First Four Daily Records for *P*3 from the Series Beginning at the Vertical Broken Line in Figure 12

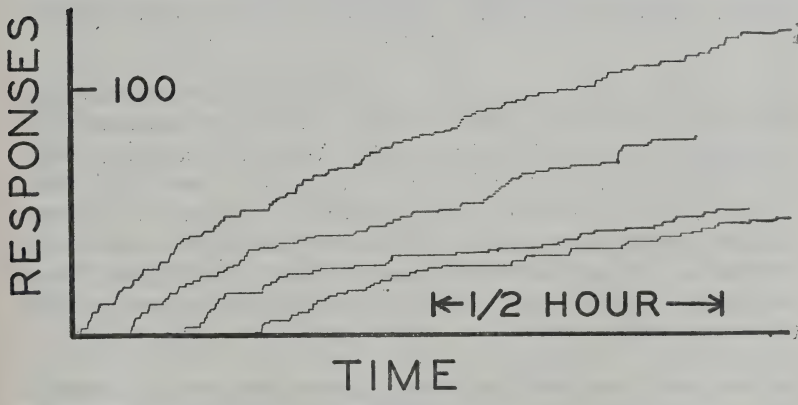

FIGURE 14

First Four Daily Records for *P*4 from the Series Beginning at the Vertical Broken Line in Figure 12

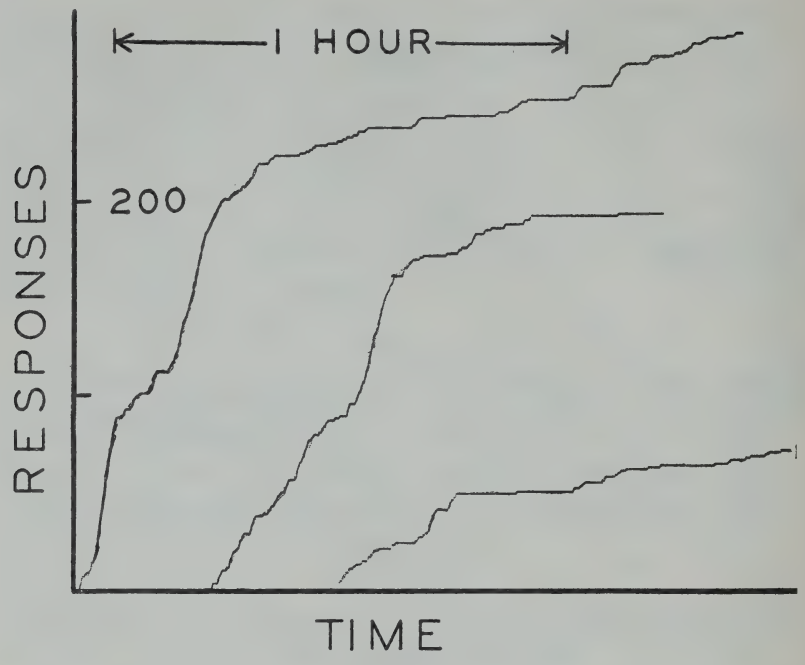

FIGURE 15

First Three Days of Extinction for *P7* after the Prolonged Periodic Reconditioning Shown in Figure 3

Note the extensive "overshooting" in the first two records with a trace in the third.

processes differ less sharply, although the decline in the total slope from day to day is much swifter during extinction. A characteristic curvature is maintained in the records. When the rate has become very low, the influence of incidental factors may be strongly felt.

Except for the difference in curvature, the records for the two processes are obviously similar. This is also true for the quantitative description. The theoretical curves in Figure 12 (lighter lines) are for the equation already given, but where the constants have the following values:

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the first period is regarded as 1¼ days, and the other points are taken as 2¼, 3¼ days, and so on. This procedure is based upon the assumption that the important variable is time-in-the-experimental-box. The assumption may need modification when the nature of the daily record (including the question of "loss of extinction") is better known, but it is sufficient for our present degree of approximation.

	P3	*P4*	*P5*	*P6*	Av.
K	215	240	560	335	338
C	155	70	180	145	138
c	20	15	25	0	15

The validity of this description is likewise dependent upon the considerations noted above.

As in the case of discrimination, there are frequent examples of overshooting. As we shall see in Appendix II, the departure of the first day from the theoretical curve must be taken as an example. These departures depend in part upon the basic rate assumed under periodic reconditioning, but they are also related to the length of time during which the rat has been subject to the procedure of reconditioning. In Figure 15 the first three days of the extinction of the reflex in rat *P7* are represented. The process was begun after the prolonged periodic reconditioning shown in Figure 3. It will be seen that during the first 20 minutes indicated in the first record the rat pressed the lever more than two hundred times. That this was, however, a temporary "strained" state is shown in the period of compensation that followed, during which the rat responded only desultorily for a period of more than an hour, and at the end of which the curve was probably close to its theoretical position (it is obviously difficult to infer the "true" course of the record). The transition from the high rate to the depression that follows is remarkably smooth. A similar overshooting occurs on the second day, and there is a trace of it on the third. Although these examples are extreme, they demonstrate the properties of the effect more adequately than the instances shown above, where the transitional stage is brief and usually obscured.

We should not expect the daily records obtained with this procedure to have the properties of the curve for original extinction (6), for we cannot legitimately assume that the intervening period of reconditioning has been without effect. In the present series the greater part of the curvature for the first day is, in fact, not due to extinction but to overshooting (see Appendix II). We have to show, however, simply that the curves given by the end points of successive daily records are similar in the two cases; and, on the assumption that compensation for overshooting or for any similar effect is complete, we may disregard the character of the daily curve altogether.

Since the curves in Figures 9 and 12 are fitted reasonably well

with a single equation, it should be possible, with the use of suitably chosen additive and multiplicative factors, to superpose them. If they are transferred to semi-logarithmic coordinates, the fit of the equation can be tested at the same time by superposing both sets upon a single straight line. This has been done in Figure 16, where the constants C and c have been reduced to zero, and K has been converted to 500 as a convenient and typical slope. The points represent the averages of the four sets of values in each experimental group. The open circles are the data for discrimination (Figure 9); the half-shaded circles are for extinction (Figure 12). Note that the latter occur at 1¼, 2¼ ... hours, as explained in footnote 14. The horizontal lines through each point mark off twice the standard deviation.

X

To the extent that the points in Figure 16 fall on the same straight line, the equation is a satisfactory description of both processes. But

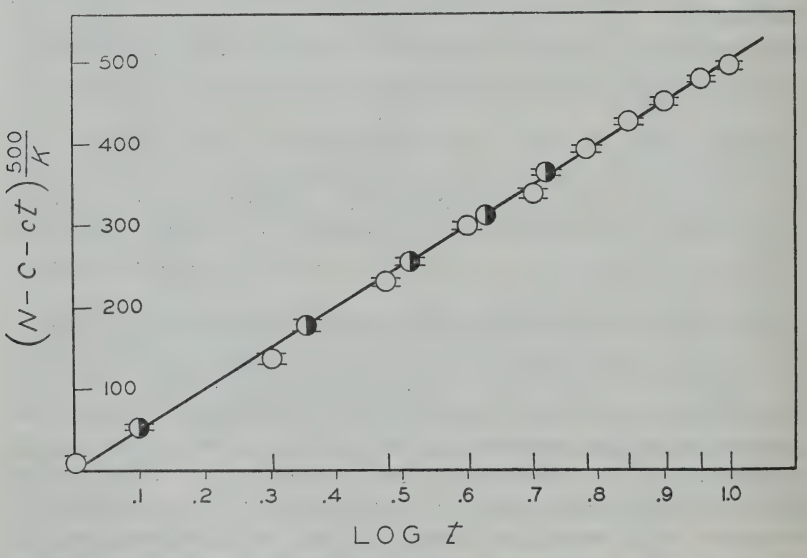

FIGURE 16

END POINTS FROM THE DAILY RECORDS IN FIGURES 9 AND 12, MODIFIED BY THE ADDITION AND MULTIPLICATION OF SUITABLE FACTORS AND SUPERPOSED UPON A SINGLE STRAIGHT LINE UPON LOGARITHMIC COORDINATES

The horizontal bars mark off twice the standard deviation of the points.

it should be emphasized that it is an empirical description only. No theoretical significance is attached to any of the constants. There are, in fact, several difficulties inherent in the equation as it stands. For example, it does not express the character of the curve as a departure from a basic rate, which is also a limiting rate. But, while it will need ultimate revision, the equation is at present adequate. In the system with which we are dealing there are other parameters of which account must be taken, and until they have been at least superficially examined any attempt at a theoretical interpretation may be suppressed.

The empirical equation is useful since, in view of the reasonably good fit demonstrated in Figure 16, it is possible to express differences between the two processes simply in terms of the values of the three constants. As we have already noted, we are ultimately interested in the first derivative, where C will have disappeared. Of the other two constants c takes a small value experimentally and does not differ appreciably between groups. This leaves K as the only notable difference. The average values of K are 565 for discrimination and 338 for extinction—a significant difference, although there is considerable individual variation, with some overlap. Part of the difference may be explained in the following way. It may be seen from an inspection of the figures that the value of K is related to the basic slope assumed under periodic reconditioning; for example, the curves showing greater slopes for days 1 and 2 in Figures 9 and 12 also have greater slopes during the subsequent change. The average slopes for the first two days in Figures 9 and 12 are 59° and 51° respectively, and we should therefore expect a lower slope in the latter case after the change has begun. But these values will not account for the whole difference in the values of K obtained above, and we must therefore conclude that K is characteristically higher during discrimination than during extinction.

Let us return to the notion of an ideal series of all pairs of stimuli. We now have records of discrimination at three representative points in such a series. First, as we have already said, the simple procedure of periodic reconditioning may be regarded as identical with that of a discrimination in which there is only a subliminal difference between the stimuli. Secondly, the curve for extinction (as given herein) will obtain where the two stimuli differ so completely that

their mutual interference is negligible.[15] And, thirdly, we have a pair of stimuli, composed of the lever alone and the lever-plus-light-or-sound, which lies midway along the series. The records obtained in the first and second cases describe, respectively, the upper and lower limits for curves of this sort. The third gives a probably typical intermediate position.

It is significant that the two limiting curves are obtained with pairs of stimuli taken from the ends of our series. To this extent there is a demonstrable correspondence between the position of a given experimental curve among other curves and the position of its appropriate pair of stimuli among other pairs of stimuli, and it is a reasonable assumption (which, indeed, we have already implied) that the correspondence holds throughout.[16] While our empirical equation is not theoretically adequate near the upper limit of the experiment range, we may still express the position of the curve for discrimination obtained in the present case by giving its value of K. Assuming this or a similar practice to be valid, what we may hope to obtain eventually is an ordering of pairs of stimuli according to the values of K that they yield.[17] This result would immediately afford an answer to most of the questions in connection with which the method of discrimination is now employed.

Aside from the use of K in determining what constitutes a significant difference in a property of a stimulus, the present result has an important bearing upon the nature of the problem of discrimination itself. Primarily it confirms our hypothesis that the process of discrimination is only a special case of extinction. But it also enables us to define the problem a little more closely. We have seen that in a discrimination if it were not for the periodic reinforcement of a related reflex the strength of the unreinforced reflex would decrease

[15]This might be the case, for example, if, instead of the simple addition of a light, the location of the lever, its shape, direction of movement, and so on could all be changed whenever the response was reinforced—in other words, if there could be two quite different levers, alternately available, the response to one of which was reinforced, the response to the other not.

[16]If we examine the procedure with which we arrange pairs of stimuli "according to the degree to which they possess their properties in common," we may find that our definition of a significant or differentiating property rests ultimately upon the rôle of the property in establishing a discrimination (if not by the organism at least by the experimenter). The correspondence that we have noted would then become virtually an identity.

[17]Any number of pairs of stimuli may, of course, occupy a single position in the series (if they possess the same value of K).

according to the first derivative of the curve for extinction. The *observed effect* of the reinforcement is the presence in the curve (for discrimination) of a number of extra responses per interval. These additional responses may be said to be due to the reinforcement of the related reflex in the sense explained in Appendix II. But (as we imply in saying that the modification takes the form of an increase in K) the number of these additional responses per interval (and therefore per reinforcement) does not remain constant during the process. The extent of the interference, to be exact, varies as the strength of the reflex undergoing extinction. Since the problem of discrimination arises because of the mutual influence of two related processes, it should be especially concerned with the nature of their interference. We may now regard the interference as in itself a process, and we may deduce some of its properties from the above discussion.

XI

Some explanation of the present method may now be required. In general, its characteristics have been determined by the subject-matter. As we have seen, a discrimination inevitably requires the alternate reconditioning and extinction of two related reflexes, so that some form of periodic reconditioning is a part of any available procedure. The *regularity* of the period in the present case is determined by the further consideration that we wish to use a rate of elicitation as a measure of reflex strength. Thus, while the use of "periodic reconditioning" may at first seem to be artificial or unnecessary, it is actually only an attempt to deal as directly as possible with a basic condition of this type of experiment. The objection that the regularity may introduce an artifact through "temporal conditioning" is met by the control curves in Figure 7 and by the theoretical remarks in Appendix I. Fortunately the baseline provided by periodic reconditioning is rectilinear, but the method would have been equally valid if it had proved otherwise.

The complexities of the account are likewise chiefly due to the subject-matter, and if they do not appear in an investigation with another method it is probably the fault of the method. Difficulties in fitting and interpreting curves cannot arise until reproducible curves have been obtained. Likewise, if the several orders of deviations peculiar to a discrimination appear here for the first time, they are none the less real phenomena; and any investigation in which they do not appear may be only deceptively simple.

One aspect of the present method may deserve comment, namely, its freedom from personal interpretation. Ordinarily the presence of the experimenter is not required during the experimental hour. Where periodic administrations of food are required, an exception must, of course, be made. But even then it may still be said that between the actual behavior of the rat and all the figures in the present paper there intervene only mechanical or mathematical steps.

Summary

(I) Technically the establishment of a discrimination consists of the continued reinforcement of one reflex and the concurrent extinction of another, where the two stimuli possess properties in common but also differ in some significant respect. The two processes cannot go on independently, and the problem of discrimination arises because of their mutual interference. The present paper examines the nature of this interference and tests the expected similarity of properties of the two processes.

(II) The apparatus consists of a suitably controlled experimental box in which there is a small lever, which may be pressed downward by a rat. The movement of the lever may or may not be followed by the delivery of a pellet of food to the rat, according to the wish of the experimenter. The box also contains an extra source of stimulating energy under the control of the experimenter, either a light or a sound, to be used as a differentiating stimulus in establishing a discrimination.

(III) If the response to the lever is periodically reinforced with a pellet of food (say at an interval of five minutes), while all intervening responses are extinguished, the successive separate extinction curves recorded with a method previously described eventually fuse, and a constant rate of elicitation is assumed. (IV) This rate may be maintained for as long as 24 experimental hours. (V) There are three orders of local deviations from such a straight line peculiar to the records and a fourth order common to all records obtained with the method. The four orders are described. (VI) The constant rate of elicitation is a function of the frequency of reconditioning. (VII) The constant rate is due not only to the summation of separate extinction curves but to a factor which depresses the rate immediately after a reconditioning. The depressive effect can be demonstrated by a modification in technique.

(VIII) The procedure of periodic reconditioning may be con-

verted into one of discrimination by adding extra stimulating material whenever the response is to be reinforced. As soon as this is done the rate assumed under periodic reconditioning begins to decline and the resulting records describe curves that are fitted with the equation

$$N = K \log t + C + ct$$

where N = number of responses, t = time in hours from the change in technique, and K, C, and c are constants. (IX) Curves obtained for pure extinction after prolonged periodic reconditioning are also described with the same equation. The two sets of data are tested by adjusting the values of the constants and superposing both sets upon the same straight line on semi-logarithmic coordinates.

(X) Although the equation is purely empirical and no significance is attached to the constants, the effect upon the curve for extinction of the concurrent reinforcement of a related reflex can be expressed as an increase in the value of the constant K. The upper and lower limits between which a curve for discrimination may lie are described. In the lower part of the range the position of a curve is expressed by K. The position is probably determined by the degree to which the properties of the two stimuli in the discrimination possess their properties in common. K may thus be used in determining what constitutes a significant property.

The extent of the effect of the reconditioning of a related reflex upon extinction varies as the strength of the extinguished reflex. The problem of discrimination is peculiarly concerned with this variation in the extent of the interference.

(XI) Reasons for adopting the method are explained.

Appendix I

Deviations of the third order may be regarded as a sort of discrimination in the following way. The rat becomes conditioned not to respond to lever + recent-stimuli-from-ingestion but to respond to lever + recent-period-of-no-responding. Here there are two groups of stimuli possessing the stimuli arising from the lever in common. The differential behavior of the rat may therefore be said to be an example of a discrimination. This is perhaps an adequate description of the behavior, but it is definitely objectionable on systematic grounds, since it requires that an interval of time be regarded as a stimulus, or at least as the differentiating member of a group of stimuli. A stimulus, as an event, takes place in time, but time

alone regarded as a stimulus cannot, of course, have any meaning. The present interpretation of deviations of the third order does not commit this error, since the interval is regarded as one during which an inhibitory state disappears, where the length of the interval is determined by the initial intensity of the state.

In addition to this theoretical objection, the present experimental result leaves no choice between the two interpretations. The development of deviations of the third order, notably in the case of *P*7, is not acompanied by any significant decline in the total slope. This is consistent with the repeated observation that an inhibitory effect is always compensated for by a later period of hyper-activity, but it is directly opposed to the present finding that the development of a discrimination involves an extensive change in rate.

Appendix II

Another way of expressing the constancy of the rate assumed under periodic reconditioning is to say that the effect of the reinforcement of a single response is constant. As we have already noted, the number of elicitations observed during the first interval in a series (as recorded, for example, in Figure 2)[18] is obviously less than the number due to the initial reconditioning, since the extinction curve is clearly interrupted; and this is also true for a few succeeding intervals.[19] The rate of elicitation is rapidly accelerated, however, until a given constant rate is attained. If we attribute the acceleration, as we have done, to the eventual appearance of the responses remaining in the interrupted curves, then the attainment of a constant rate must be taken to mean that the number of responses now observed in a single interval is precisely the number due to the reconditioning of one response. The output is just balancing the input, and there are no unexpressed responses to cause further acceleration.

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[18]This figure is a better illustration than the records for the first day in Figure 3, because the periodic reconditioning is begun in the latter case immediately after the release, and the loss of extinction normally observed at that time tends to obscure the fusion of the separate extinction curves.

[19]A response is said to be *due to* previous conditioning in the sense that it would not have been observed if the conditioning had not taken place. We must resort ultimately to a statistical treatment to prove this relation, since a few "investigatory" responses (occurring linearly in time) are present as a base-line. For any reasonable degree of approximation, however, a mere inspection of the present records will suffice. In Figure 1, for example, the relation between the four curves and the times of reconditioning is perfectly clear.

We may represent this relation with the ratio N_e/N_c, where N_e is the number of responses in the extinction curve and N_c the number reconditioned. In the present case the value of the ratio is constant during an extended experimental series. From the data in our possession we can calculate the value when $N_c = 1$. Dividing the total number of responses in each record in Figure 3 (10,700, 3980, 2420, and 2200) by the number of minutes in the series (1440), we obtain average rates of 7.10, 2.51, 1.57, and 1.45 responses *per minute* for rats *P*7, *P*9, *P*8, and *P*10 respectively. If we now multiply by the number of minutes in the average interval at which each was observed, we obtain 22.8, 16.1, 15.2, and 18.8 as the number of responses *per interval.* In order to take account of uncompleted intervals at the end of each experimental period, we may assume a complete compensation on the following day and obtain another set of values by dividing the total number of responses in each series by the total number of reinforced responses.[20] The resulting values are approximately the same: 22.8, 17.4, 15.1, and 18.4. From the first method a mean of 18.2 ± 1.7 is obtained; from the second, 18.4 ± 1.6. Similarly in the experiment represented in Table 3 we may convert the values for the tangents of the slopes into responses *per hour* by multiplying by 255, the number of responses *per hour* at which the tangent equals 1. This yields 329, 281, 239, and 181 responses *per hour* for the four intervals of 3, 5, 7, and 9 minutes respectively, and dividing by the number of times each interval is contained in 60 minutes, we obtain values of 16.5, 23.4, 27.9, and 27.1 responses *per interval.* It has been noted that the value for the three-minute interval is probably too low and the others too high, and this is in agreement with the mean obtained above. Within rather wide limits, therefore, the ratio is roughly independent of the frequency of reconditioning.

It may be concluded, then, that for every response experimentally reinforced, about 18 will be observed without reinforcement. But this value of the ratio N_e/N_c is not universal. It may hold approximately for the original conditioning of the response, as will be shown elsewhere, but it may also be greatly exceeded in subsequent reconditionings. Furthermore it does not hold when N_c is more than

[20]There is no reason for subtracting the number of reinforced responses from the total before dividing.

one. Thus if *two* successive responses are periodically reconditioned, the rate of elicitation is increased above that obtained from the reconditioning of one response at the same interval, but it is by no means doubled. This loss in efficiency increases rapidly as N_c increases. The value of the ratio is also certainly dependent upon the degree of hunger present during either the conditioning or extinction (see above), upon the temporal relation of the two processes, and doubtless upon other factors. The relationship is, in fact, even more complex than we have here presented it. In speaking of an N_e/N_c ratio we imply that the curve for extinction is composed of a finite number of responses, although, if our description of the process is essentially correct, this is not true. For certain purposes, however, it is possible to bring the curve to an arbitrary end. But we are then still dealing with only the height of the curve and are neglecting the other information supplied by its shape or the area under it.

Fortunately a description of the relation between what may be called the amount of extinction and the amount of the conditioning to which it is due is not essential to the argument of the paper. This appendix has been included only because the constant rate observed during periodic reconditioning is the clearest indication so far obtained that there is in fact a definite quantitative relation. The indication is clear enough to warrant this tentative general principle: *the total number of elicitations of a given response is limited.* The principle is to a certain extent already implied by the nature of the concept of the reflex (2). Thus, while the elicitation of a response is determined primarily by the presence of the appropriate stimulus, given the latter, it is further controlled by laws the nature of which we have considered elsewhere (2, 3, 5, *et al.*) and of which we are here concerned with several specific examples. It is characteristic of these laws that they establish envelopes, from which the behavior of the rat cannot escape, although it may for many reasons fall short of its maximal development thereunder. In general, the envelopes are experimentally controllable, and in the present special case of conditioning followed by extinction the extent of the envelope for extinction is seen to be determined by the amount of conditioning.

The instances of "overshooting" reported herein and elsewhere (6) may seem to violate this general rule. Consider, for example, the record for *P*5 in Figure 12. We have already supposed that

the constant rate maintained on days 1 and 2 in this graph is determined by the balanced input and output obtaining under periodic reconditioning. Now, beginning with day 3 there is no longer any input. Nevertheless, during the first part of this day a much higher rate of elicitation is actually observed. Where are these extra responses obtained?

This question can be answered from the experimental material and the hypotheses already in our possession. We saw above that the constant rate is determined not alone by a mere summation of extinction curves but by the inhibitory effect of the ingestion of food. But no food is ingested during extinction, and the rate of elicitation, free from this restraining effect, may rise above its previous constant value. *Nevertheless,* we must still regard the total slope as a measure of the effect of the reconditioning and any increase above it must be supposed to be a temporary state of imbalance. In other words, the curve for extinction *must* start at the constant slope given during periodic reconditioning. It is significant therefore that the theoretical curves in Figure 12 satisfy this requirement, although the property was not sought in the curve-fitting. According to this explanation the phenomenon should not be observed with the procedure of discrimination, since the inhibitory stimuli continue to be administered. It is apparent from Figure 9 that this is true, since the initial rate during discrimination is the rate previously assumed under periodic reconditioning. Our demonstration of the essential similarity of the properties of the two processes (based as it is upon the end points of the daily records) does not extend to the nature of the daily records themselves or, consequently, to these temporary "strained" states.

Although in this case the overshooting can be attributed to the removal of inhibitory stimulation, it is not possible at present to account for all examples in a similar way. Nevertheless, they can be shown to be exceptional states. In the present case we have demonstrated this by showing a return to a theoretical curve. In other cases of overshooting we can usually show that a record returns to the extrapolation of a curve previously established. It is thus possible, even though a definite cause of a deviation is lacking, to demonstrate that it is only a temporary state of imbalance and thus to preserve the general principle, while admitting occasional violations.

Appendix III

The average delay in pressing the lever after the magazine has been connected (see Section IV) should be a function of the rate of elicitation. More exactly it should be equal to one-half the average interval between successive responses, provided the time of connection is wholly independent of the behavior of the rat, and provided the responses to the lever do not occur in groups. Since the second of these conditions is not fulfilled, the observed delays are considerably longer than half the average interval. Table 4 gives the relevant material from the experiment represented in Figure 3. Lines 1-5 are taken from measurements made at the time of the experiment to the nearest five seconds (a closer approximation was impractical at the time). Line 2 is to be preferred to line 1, since a constant rate was not stabilized on the first day. It should be compared with line 7 in estimating the departure from the values to be expected when the above conditions are fulfilled. A progressive shortening of the delay may be observed in a comparison of lines 3, 4, and 5. This is due to the appearance of third-order deviations, which tend to concentrate the responses at the end of the interval, where the reconditioning occurs. The connection of the magazine is then made during a period of relatively rapid responding; and where the third-order effect is pronounced the observed delay may

TABLE 4

	Interval between periodic reinforcements			
	3 min.	6 min.	9 min.	12 min.
1. Average delay for total series in seconds	5.6	26.7	47.0	59.3
2. Average, omitting first day, in seconds	5.0	23.6	40.6	59.6
3. Average for first eight days in seconds	9.5	34.9	67.6	93.5
4. Average for second eight days in seconds	4.6	24.7	38.1	43.6
5. Average for third eight days in seconds	2.8	20.9	37.4	38.0
6. Average rate of responding for total series in responses per hour	446	166	101	92
7. One-half the average interval between responses in seconds (calculated)	4.0	10.9	17.8	19.6
8. Number of delays counted	439	208	136	92

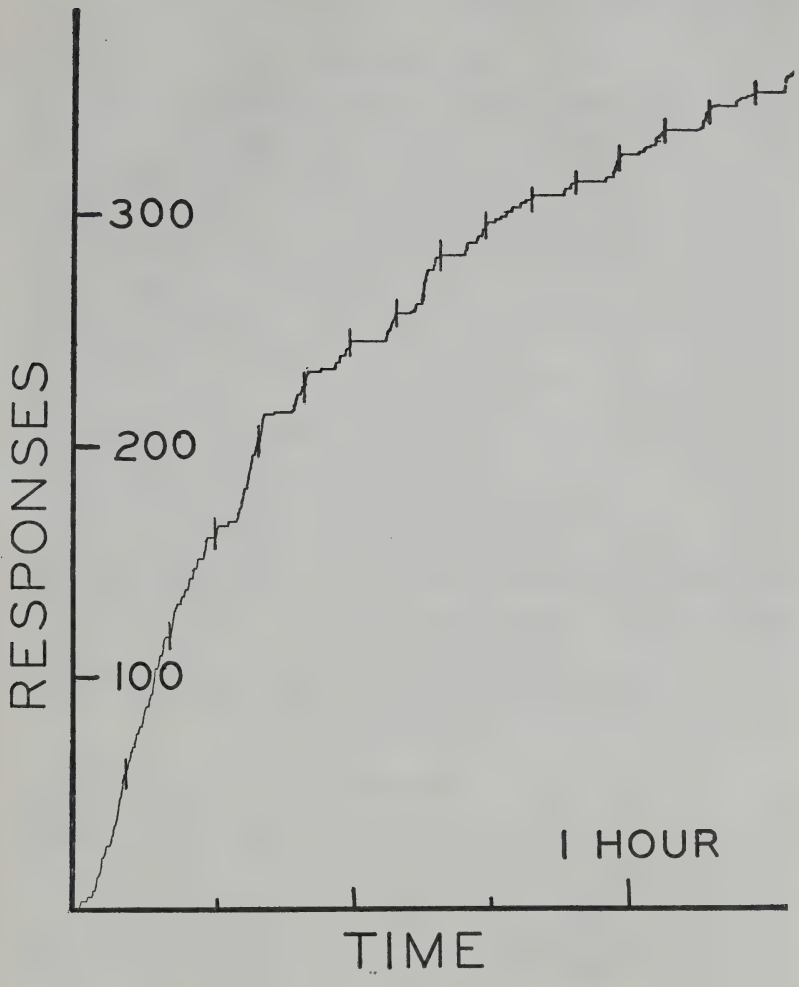

FIGURE 17

CHANGE IN THE SLOPE ASSUMED UNDER PERIODIC RECONDITIONING WHEN THE ADMINISTRATION OF PELLETS IS NOT CORRELATED WITH RESPONSES TO THE LEVER

Pellets are received periodically as usual but no response is reinforced.

become less than that calculated as half the average interval (cf. column 1, lines 3, 4, and 5).

Appendix IV

As an extra check upon the present interpretation of a discrimination, we may examine a third case in which the slope established under periodic reconditioning should change in approximately the same way. Let a basic slope be established with any suitable interval of reconditioning. Then let one pellet of food be discharged from the magazine periodically at approximately that interval, but always at a time when the rat is not responding. Then, although pellets are obtained as usual, none of the responses to the lever is actually reinforced. Figure 17 shows the result for the first day of such an experiment. A basic slope had been established with reconditioning at five minutes for a long series of daily experiments. On the day represented in the figure a pellet was discharged from the magazine by the experimenter approximately every five minutes, but never less than fifteen seconds after the last previous response of the rat. A very significant change in slope is apparent.

This is the only example that has so far been obtained with this procedure. The series of which it is the first member could be described with the equation given above, although a single case is hardly convincing. Granting the applicability, however, the series shows a large value for K, which seems to indicate that some reconditioning is effected even though the discharge of the pellet follows the preceding response at a considerable interval.

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LA VITESSE DE L'ÉTABLISSEMENT D'UNE DISCRIMINATION

(Résumé)

Une méthode utilisant la vitesse à laquelle un réflexe se montre sous une stimulation constante comme mésure de sa force pendant les processus du conditionnement et de l'extinction s'emploie dans l'investigation de l'établissement d'une discrimination. Un réflexe arbitraire—la poussée d'un levier en bas par un rat—devient premièrement conditionnel quand la réponse est immédiatement suivie de la mise de nourriture dans un plateau commode et est éteint quand la réponse n'est pas ainsi suivie. Dans le cas présent on conditionne de nouveau le réflexe et l'éteint alternativement en permettant que la résponse ne soit suivie de la mise de nourriture qu'à certains intervalles fixes. Les courbes séparées de l'extinction se combient alors, et le réflexe assume une force constante, qu'il maintient sans une modification signifiante pendant même vingt-quatre heures expérimentales. La valeur de la force assumée est une fonction de l'intervalle auquel le réflexe devient de nouveau conditionnel. Aux plus hautes vitesses d'établissement (c'est-à-dire, aux plus courts intervalles) la mise de la nourriture inhibe à la longue le réflexe pendant une partie de l'intervalle succédant, mais un effet compensateur fait que le nombre total des résponses reste le même pour chaque intervalle. Dans l'établissement d'une discrimination on introduit un stimulus supplémentaire à chaque nouveau conditionnement mais on l'omet pendant les périodes intermédiaries de l'extinction. La réponse au levier plus le stimulus supplémentaire reste alors tout-à-fait conditionnelle, tandis que la réponse au levier seul s'éteint. La courbe qui décrit ce changement a les propriétés de la courbe normale de l'extinction, dont l'on donne des exemples. Une différence quantitative mésure probablement l'empiétement des deux groupes de stimuli.

SKINNER

DAS MASS FÜR DEN VOLLZUG EINER UNTERSCHEIDUNG

(Referat)

Eine Methode, die die Angaben verwertet, unter denen ein Reflex unter konstanter Reizung zustandekommt, und die als Mass deren Stärke während der Erstellung bedingter Reize und der Auslöschung gelten, wird zur Untersuchung des Unterscheidungsprozesses gebraucht. Ein beliebiger Reflex —das Herunterdrücken eines Hebels durch eine Ratte— wird ursprünglich bedingt, wenn unmittelbar nach der Reaktion Nahrung in einem bequemen Trog verabreicht wird, und ausgelöscht, wenn die Reaktion nicht so befolgt wird. Im gegenwärtigen Fall wird der Reflex abwechslungsweise bedingt und ausgelöscht, indem man der Reaktion nur nach gewissen festgelegten Intervallen Nahrung folgen lässt. Die einzelnen Auslöschkurven verschmelzen dann, und der Reflex nimmt eine konstante Stärke an, die ohne bedeutende Änderungen bis auf 24 Experimentstunden erhalten bleibt. Der angenommene Stärkewert ist eine Funktion des Intervalls, unter dem der Reflex bedingt wurde. Bei höheren Werten der Hervorlockung (d. h. bei kürzeren Intervallen) hemmt die Verabreichung von Nahrung gelegentlich den Reflex für einen Teil der nachfolgenden Intervalle, aber eine Kompensationswirkung lässt die Gesamtzahl der Reaktion pro Intervall unverändert. Während der Erstellung einer Unterscheidung

wird bei jedem Weiderbedingungsvorgang ein Extrareiz eingeführt, der aber während der dazwischenliegenden Auslöschperioden unterlassen wird. Die Reaktion auf den Hebel plus Extrareiz bleiben ganz bedingt, währenddem die Reaktion auf den Hebel allein ausgelöscht wird. Die Kurve, die diese Veränderung beschreibt, hat die Eigenschaften einer normalen Auslöschkurve, wofür Beispiele gegeben werden. Wahrscheinlich ist ein quantitativer Unterscheid ein Mass für das Übergreifen der zwei Reizgruppen.

SKINNER

JUNE ETTA DOWNEY

July 13, 1875—October 11, 1932

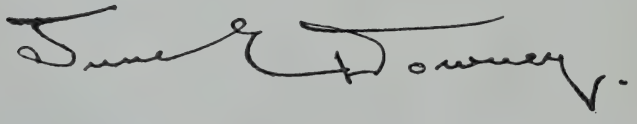
June Etta Downey, Professor of Psychology and Philosophy at the University of Wyoming, died October 11, 1932. She became ill while attending the Third International Congress of Eugenics in New York, in August, and was removed to the home of a sister in Trenton, New Jersey, where death followed an operation.

Born in Laramie, Wyoming, July 13, 1875, she resided there throughout her life. She came from a pioneer family that contributed much to the development of the state. Her father, Colonel Stephen W. Downey, was one of the first territorial delegates to the United States Congress from Wyoming. It was largely through his efforts that the University of Wyoming was established. For many years he was president of its board of trustees. Her uncle, Mr. W. O. Owen, was a famous mountain climber who was the first to scale the Grand Teton in western Wyoming.

Dr. Downey, too, was a pioneer, who spent her life on the frontier that lies between what is known and what is unknown in the field of personality measurement. Her researches, dealing with the problems of aesthetics, handwriting, muscle-reading, will-temperament testing, color-blindness, types of dextrality, imagery, and personality earned for her international recognition.

The early education of Dr. Downey was obtained in the Laramie Public Schools and in the University Preparatory School. She was graduated from the University of Wyoming in 1895. At that time she was interested primarily in literature and languages. Throughout life she was a proficient scholar in Latin and Greek, and read French and German with great facility. She was introduced to many of the concepts of natural science by the late Dr. Edwin E. Slosson, who was Professor of Chemistry at the University of Wyoming from 1891 to 1904. Their mutual interest in literary expression led to collaboration on a volume entitled *Plots and Personalities,* which was published in 1922.

After graduating from college, Dr. Downey taught for a year in the public schools of Laramie. She enrolled as a graduate student at the University of Chicago in 1896, and received the master's degree in 1898. During that period she published her first article in a psychological magazine (1). Although her interests were broad,



she continually returned to a consideration of the problems of aesthetics. Her last book, *Creative Imagination,* published in 1929, summarized the results of thirty years' investigation in that field.

Dr. Downey returned to the University of Wyoming in 1898 and taught English and Philosophy. In 1901 she attended the summer session at Cornell University and was introduced to the experimental procedures of Edward Bradford Titchener. Undoubtedly that contact did much to crystallize her interest in the problems of psychology. The courses that she gave in Experimental Psychology in succeeding years showed clearly the influence of the laboratory manuals and writings of Professor Titchener. She was more interested in motor processes than in sensory phenomena. Her teaching was further enriched through attendance at summer classes at Columbia University, and by means of visits and testing opportunities at the Boston Psychopathic Hospital.

In 1905 Dr. Downey was made Professor of Philosophy at the University of Wyoming. A year later she was granted a fellowship in the Department of Psychology at the University of Chicago. She completed her thesis, *Control Processes in Modified Handwriting: An Experimental Study* (2), and received the degree of Doctor of Philosophy in 1907. Her graduate work was done under the direction of James Rowland Angell. Graduate students who were associated with Dr. Downey and who served as subjects for her experiments included Walter Van Dyke Bingham, Florence Richardson Robinson, and Clarence Stone Yoakum. Professor Angell and John B. Watson also cooperated in her study of handwriting. Facsimiles of their signatures, written under various forms of distraction, were reproduced in the dissertation. In concluding her study of *Control Processes in Modified Handwriting* Dr. Downey wrote, "The main difference between subjects was the extent to which writing was automatized and the degree to which the reagent allowed conscious direction to lapse" (2, p. 143). Dr. Downey's early contacts with leaders in the field of psychology were maintained throughout life by correspondence and attendance at meetings of the American Psychological Association.

After completing her graduate work at the University of Chicago, Dr. Downey was made head of her department and continued as Professor of Philosophy at the University of Wyoming until 1915, when she was made Professor of Psychology and Philosophy. She was the only woman head of such a department in a state university

in the country. She continued in that position until her death in 1932. Her position was attained as a result of sound classical and scientific training, and was supported by her administrative skill. She was a quiet, efficient worker, who did much to shape the educational policies of her Alma Mater. She served her university in many important capacities. In addition to her teaching duties she acted as Principal of the Department of University Extension from 1908 until 1916. For years she was Chairman of the Graduate Committee. The number of graduate students increased from three to over two hundred. Her high ideal of scholarship never was sacrificed in the face of expanding student enrollment. Practically her entire life was devoted to the service of the University of Wyoming. Only twice did she accept offers to give courses elsewhere. In the summer of 1920 she taught at the University of Chicago; in the summer of 1930 she lectured at the University of Iowa.

Dr. Downey was not a person of great physical energy, but, in spite of the rigors of Wyoming winters, with snow falling intermittently from September until May (Laramie has an altitude of 7100 feet), she met her classes regularly and promptly. She had a high regard for her obligations to her students. She was an inspiring teacher who retained her enthusiasm to the end. Her distinguished student, also a native of Laramie, Professor John E. Anderson of the University of Minnesota, has said: "Two characteristics of her teaching were striking; first, her brilliance and breadth of interest, second, her unbounding enthusiasm for psychology and its significance for human affairs. The first stimulated and attracted able students; the second communicated to her students, resulted not only in an enthusiasm for psychology but a willingness and desire to read widely and to experiment. Always there were interesting things going on in the laboratory, many of which did not find their way into print. Moreover, there was always a feeling of freedom in following individual interests, rather than the imposition of a stereotyped approach to problems." [1]

During the academic year Dr. Downey carried a full teaching load, yet found time to pursue many interesting lines of investigation. She worked with a minimum of professional contacts. In commenting upon her achievements, Professor Anderson has written: "Her

[1]Contained in a letter to R. S. U.

accomplishment is all the more remarkable when these limited facilities are taken into account. Despite relatively unfavorable surrounding circumstances she maintained a high level of productivity long after her reputation was well established." [2] In the months preceding her death she cooperated with Professor H. C. Warren in preparing terms on graphic functions for the *Dictionary of Psychology* which was developed under his direction.

Dr. Downey allied herself with no single school of thought, but sought constantly to know and evaluate each new viewpoint and approach to psychological problems that was propounded. She had a remarkable sense of relative values. Her excellent philosophical background prevented her from viewing any system of psychology as final. Her own mental growth never ceased.

The publications of Dr. Downey challenged the attention of psychologists for thirty years. Her early interest in the problems of imagery persisted throughout life, and lent unity to all of her work. Her own imagery was vivid, as is indicated in her volume of poems, *The Heavenly Dykes.* Her scientific interests led her to an examination of the creative process, and resulted in a frequently quoted study, *The Imaginal Reaction to Poetry* (5). She sought the key to the problem of personality differences by way of the study of images. She has said: ". . . the reports of competent introspectionists encourage the belief that when experimental ingenuity has solved the problems of objective control we shall make great discoveries as to the practical significance of even slight imaginal differences" (10, pp. 37-38).

She was interested in what she described as "the variational factor in psychical experience" and early became impressed with the possibilities of using specimens of handwriting as expressions of personality differences. While most of the American psychologists were learning how to give Binet tests in the decade before 1920 she experimented with various methods and devices for measuring traits other than those usually considered in intelligence testing. Although her scale for the individual measurement of "will-temperament" traits was not published until 1919, important studies upon which the scale was based were reported in 1908, 1909, 1915, and 1919. While others were developing the intelligence-test scales, she was working, practically alone, in the field of temperament-trait testing.

[2] Contained in a letter to R. S. U.

Her studies of imagery and graphic functions paralleled the development of mental-test theory and statistical analysis of test results.

Muscle-reading engaged Miss Downey's attention in 1907. She became remarkably skilled in responding to the involuntary cues provided by psychologically unsophisticated guides who concentrated on concealed objects. Her interest in the identification of mental types led her to perform a series of experiments with sixty subjects. In these she reported 70 per cent success at the time of the first trial. If the first and second trials are considered, she succeeded with 93 per cent of her subjects. She failed with two of the sixty cases. She reported marked individual differences in motor impulsion, resistance to opposition, motor inhibition, coordination, and perseveration (4). The Individual Will-Temperament Test represented an attempt to measure these traits by the use of paper and pencil rather than through the relatively elaborate and subjective methods involved in muscle-reading.

Before utilizing handwriting exercises in tests for temperament traits, Dr. Downey canvassed the field of graphology. Her report on *Graphology and the Psychology of Handwriting* (6) is in two parts. In the first she compared the "graphological contentions and the outcome of modern scientific investigations of handwriting." In the second she reported experimental studies made in her own laboratory. She "found reason to believe that graphic size is symptomatic of the free release of energy" (6, p. 132). "The concept of the explosive versus the inhibited hand proved particularly enlightening . . ." (6, p. 133). She has stated: "My experiments on automatic writing where I found increase in size of writing with growing automatism were supplemented by those on disguised handwriting where decrease in size accompanied the focusing of attention on writing." [3] These studies led to the belief that handwriting might be used in testing certain dynamic aspects of personality.

The Will-Profile: A Tentative Scale for Measurement of the Volitional Pattern (7) was published as a *University of Wyoming Bulletin* in 1919. With characteristic modesty the author made little reference to the decade of preliminary work. She stated emphatically that, although handwriting was used (as a common motor activity that leaves a permanent record), *no graphological assumptions what-*

[3]Contained in a letter to R. S. U.

ever were made. The bulletin contained a sample test blank, test directions, comments on the tests, tentative norms, and an invitation to psychologists to experiment with the procedure. The Downey Individual Will-Temperament Test immediately attracted the attention of those interested in measuring the non-intellectual aspects of personality. A second printing of the bulletin was necessary to supply the demand for copies.

Dr. Downey was ingenious in setting up situations that would enable her to study the differences in graphic and other motor responses. She viewed personality as an integrated whole and reacted against the tendency to abstract a trait from its setting. She recognized "various will-temperaments ranging from that of the fluidic explosive individual to that of the deliberate individual who gets into action with difficulty." She suggested the use of such terms as "freedom from inertia," "flexibility," "motor impulsion," "motor inhibition," "speed of decision," to describe important, non-intellectual traits. As the techniques of mental testing developed, she accepted the fact that much additional work would have to be done before her test forms would be reliable and valid. She constantly worked in that direction and encouraged others to do so. She welcomed constructive criticism and made every effort to improve her tests.

The Downey conception of personality is unique. It is based upon the assumption that the traits, or habits, of the individual are integrated to various degrees. Habits also differ as to their degree of automatism. In measuring personality, therefore, it becomes important to know two things: first, how well the trait tested for is integrated with others and, secondly, to what extent the motor response has become automatic.

Individuals were grouped according to three main classifications by Dr. Downey: (*a*) the speedy, "hair-trigger," fluidic type; (*b*) those reacting in a forceful, decisive way; and (*c*) those characterized by carefulness and persistence of response. This threefold classification, however, was not rigid. The existence of intermediate types was assumed, types that may be described as mobile, mobile-aggressive, non-specific, deliberate, low-level, atypical, and psychopathic (8, pp. 76-85).

Unlike the familiar intelligence and educational scales, the Downey Will-Temperament Test does not utilize a total score. Instead, the psychographic method, advocated by Rossolimo and used by

Kraepelin, Seashore, and others, is employed. Scores are plotted on a graph, yielding a "will-profile" for each case. In interpreting "patterns" or "will-profiles," Dr. Downey was concerned with the *intra*-individual relationship of the parts. She has stated: "It is the *relative dominance* of one or another trait in the *individual's own profile* that counts rather than a group ranking on a trait. Of course, comparisons of profiles can be instituted, but so far as a pattern is concerned, the decision rests upon an inspection of the individual profile and not upon a comparison with others." [4] She also has stated: "The relative height of the different parts of the graph indicates at a glance the outstanding features. Overemphasis of the first third of graph suggests the 'hair-trigger' organization; overemphasis of the central part, a wilful, aggressive pattern; overemphasis of the last part, a slow, accurate, tenacious type. That characteristics based upon these profiles are amazingly to the point is very evident to one who knows the individuals concerned" (8, p. 35).

Dr. Downey opposed the efforts to deal with total scores on the Will-Temperament Test, and insisted that personality should be expressed in terms of a pattern. She suggested the term "will-profile" to describe the interrelationship of the personality components. A variation in one trait affected the interpretation of the entire profile. This factor has been the despair of statisticians who have considered her work. Occasionally she would use an analogy from the field of endocrinology, pointing out that a disturbance in the functioning of one gland affects the functioning of the others, and that a small difference in thyroxin, for example, may be an important determiner of the general level of mental alertness of an individual.

In interpreting a profile, the intelligence level of the individual was considered. Theoretically, two identical "will-profiles" would be interpreted differently if the subjects varied significantly in intelligence. The problem of weighting the tests in the battery remains unsolved because the Downey concept of personality requires that each trait be weighted in terms of the variations in other traits in the particular will-profile under consideration. To date, no adequate statistical techniques have been developed that would score a will-profile even if each test in the battery were a reliable and valid measure of a specific trait. In this connection she wrote:

[4] Contained in a letter to R. S. U.

"Explosive tendencies which may speed an individual of great ability on to success may ruin a less intelligent man. Inhibitions may nullify genius; they may protect a moron" (8, p. 61).

Although Dr. Downey was not the first to draw attention to personality as an integrated whole, she was the first to devise a test for non-intellectual traits in order to evaluate her theories. Subsequent research has demonstrated that writing the phrase "United States of America" is not a test for general speed, but is a highly specific item. The same is true of other portions of the original Will-Temperament Test. Nevertheless, nothing can rob Dr. Downey of her place as a pioneer in the field of personality measurement, or can detract from the credit due her for stimulating a vast amount of productive consideration of the problems of personality measurement. Her work ultimately will be viewed against the background of the whole testing movement.

Few realize that years of study and experimentation preceded the publication of the Individual Will-Temperament Test in 1919. Many of her minor researches were not published by Dr. Downey. The important thing to note is that her studies in the fields of graphology and muscle-reading revealed important individual differences of a volitional type. The attempt to measure these motor responses was in accord with the tenets of modern psychology.

The Will-Temperament and Its Testing appeared in 1923. This book brought together the scattered accounts of the work in the field. Here we find "will-temperament" and "character" defined in the following terms (8, pp. 60-61):

> The word "temperament" is used to refer to an innate, relatively permanent disposition. The word "will" is prefixed to it to indicate the nature of the particular disposition that is tested. This limitation in meaning of the term "temperament" is necessary in order to avoid a possible misunderstanding; namely, the notion that the test is concerned with emotional reactions. By "will," however, no mysterious power is to be understood; it refers merely to the dynamic pattern of the individual. The will-temperament test, since it taps chiefly innate tendencies, is only in part to be conceived as a character test.
>
> Character may be thought of as the organization of native and acquired traits effected through inner subjective factors and outer objective ones. Character, in this sense, will be influenced by intelligence or by power to reflect upon and

> discover the significance of experiences and by the capacity to form ideals; it will also be determined by sensitiveness to social control; and it will be modified by external pressure and by the acquisition of specific habits through pressure of the environment.
>
> Character is not, then, a sum-total of native tendencies: it is a product built upon these tendencies; it is the *direction* in which native tendencies are turned. It follows that *character* is not subject to measurement through tests in precisely the same way as native tendencies are, although one may take a cross section of it at any particular moment. The will-temperament determines the form assumed by character, although it does not determine its content. Similarly, the will-temperament determines the *use* that will be made of general intelligence, although it does not determine the degree of intelligence.

It is not possible to identify the sources of each of the twelve parts of the Individual Will-Temperament Test. Speed of Movement, Flexibility, Motor Impulsion, and Motor Inhibition may be traced to the experiments on modified handwriting, muscle-reading, disguised handwriting, and automatic writing. Resistance to Opposition grew definitely out of the muscle-reading experiments. The Coordination of Impulses test was influenced by Roback's study, *The Interference of Will-Impulses* (11). James's discussion of the explosive and obstructed types of will also contributed to the development of the test.

Psychologists who have used the Downey Individual Will-Temperament Test have been impressed with its possibilities in studying disparate groups, such as whites and Negroes, delinquents and non-delinquents, or psychopathic and normal subjects. It has been found valuable as an aid during the interview in mental hygiene clinics. Practically all of Dr. Downey's discussions were based upon results obtained by means of the Individual Will-Temperament Test although she published a Group Will-Temperament Test in 1922. Most of the criticisms of the tests have grown out of results obtained with the group form.

Now that all the evidence is in, it appears that the Individual Will-Temperament Test is most worthy of consideration, and that the three group forms which have appeared (Downey Group, Carnegie, and Non-Verbal) were unsuccessful attempts to adapt the procedures. The group tests have been criticized because of the amount of subjective material they contain, because the subtests are

short and unreliable, and because of the difficulties of administration and scoring which they present. The individual test can be criticized, to some extent, on the same grounds, but it has one advantage in providing a face-to-face relationship that enables the examiner to "size-up" the subject as he goes through the routine. The Individual Will-Temperament Test has distinct value as an interviewing device. It can be administered in an hour, and each applicant, or subject, is involved in the same series of situations. In the end the observer is enabled to form an opinion about the subject that may be reduced to a descriptive paragraph or a profile, as he sees fit. As a tool for training interviewers it has much to offer.

An adequate account of the Downey Will-Temperament Test must give due regard to: (*a*) the problem as defined by Miss Downey in her own writings, (*b*) the historical importance of the Individual Will-Temperament Test, (*c*) the fact that the test represented an attempt to measure some of the personality characteristics recognized and discussed by James, Binet, Roback, and others, (*d*) the secondary part played by all of the group forms of the test, and (*e*) the challenge presented to statisticians to evaluate the intra-individual relationships that exist in a given personality.

Although many honors came to Dr. Downey, she remained a modest, retiring person throughout life. She has been cited in *American Men of Science,* and her name was starred in the 1927 edition. Her achievements are recognized in *The Psychological Register, Leaders in Education, Who's Who in America, Women's Who's Who in America,* and *Who's Who in American Education.* She was a member of the American Psychological Association, and served on the Council (1923-1925). From 1924 to 1926 she was a cooperating investigator for the National Research Council Committee on Scientific Problems of Human Migration. She was a Fellow of the American Association for the Advancement of Science, and a charter member of the Colorado-Wyoming Academy of Science. She was a charter member of the Society of Experimental Psychologists. She was elected to Sigma Xi and to Phi Kappa Phi. She was a member of the American Association of University Professors, the American Association of University Women, and the American Quill Club.

Dr. Downey wrote seven books. She published approximately seventy scientific articles, numerous reviews, and many contributions in literary and non-technical magazines. She was a contributing

editor of the *Educational Measurement Review,* a cooperating editor of the *Journal of Applied Psychology,* and a member of the editorial and collaborative staff of the recently founded international quarterly review for psychodiagnostics, *Character and Personality.* She contributed the articles on "Handedness" and "Handwriting" in the Fourteenth Edition of the *Encyclopedia Britannica.*

Some of Dr. Downey's work during the past decade, as reported in "Types of Dextrality and Their Implications" (9), gave further evidence of her consideration of the problem of images. Although her major studies in the field of handedness were statistical, she suggested that type and vividness of imagery may be related to the degree of blocking that occurs in the motor responses evoked in a given situation. Consequently, she expected to find less vivid imagery among the extremely right- and left-sided individuals. Those whose unimanual and bimanual responses are of the mixed type, with accompanying conflict and blocking, would have the more vivid imagery, acording to her theory. Her studies of handedness have encouraged other investigators to work with problems of laterality of function.

June Downey found greatness in the remote mountain town where she was born. Her influence over the lives of the hundreds of students who sat in her classes will be enduring. The song, "Alma Mater," which she wrote in 1898, will be sung by University of Wyoming students for years to come. Psychologists will consider her theories and consult her writings as long as they are interested in differences in temperament. Her contributions in the field of aesthetics likewise are of enduring value. Her life was an inspiration to many. Those who knew her only through her writings recognized her literary and scientific achievements. Her many fine qualities not conveyed on the printed page could be known only to the few who were associated with her. They knew her resourcefulness, her loyalty, her whimsical humor, and her personal charm.

RICHARD STEPHEN UHRBROCK

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ON THE NEUROLOGICAL BASIS OF THE LOCALIZED RESPONSE TO IRRITANTS ON THE SKIN*

From the Physiological Laboratory of Clark University

HUDSON HOAGLAND AND LOUIS BERKOVICH

I

It was pointed out by one of us (Hoagland, 5) that cutaneous localization may depend upon the overlap of areas supplied (*a*) by single cutaneous fibers of a given nerve trunk, and (*b*) by different nerve trunks. This suggestion was based on studies of nerve impulses from single receptors in the skin of the frog and from areas supplied by whole nerve trunks (Adrian, Cattell, and Hoagland, 2; Hoagland, 4). It was suggested that the effect of such overlaps would be to produce different patterns of impulse frequencies in many fibers for slight shifts of position of a stimulating object on the skin, thus forming a peripheral "local sign." Recently Thompson and Inman (6) have demonstrated extensive overlapping of cutaneous areas supplied by different nerves in man.

II

The following experiments involving the progressive destruction of sensory endings in frogs' skin were undertaken primarily with the view of testing the foregoing hypothesis concerning the peripheral basis of cutaneous localization.

Fifteen-per-cent acetic acid destroys the sensory endings in frogs' skin after some minutes of application. This is readily demonstrated by the complete failure of nerve impulses in about five minutes after application of the acid. A Matthews amplifier and oscillograph in conjunction with a camera, a standing wave screen, and a loud speaker were used in the present series of experiments when it was desired to examine the discharge of nerve impulses from a cutaneous area [cf. Hoagland (4) for a description of the procedure]. With the destruction of the pattern of endings localization of the response of the frog to the irritant was expected to decline progressively.

*Accepted for publication by Carl Murchison of the Editorial Board.

TABLE I

RESPONSE OF A FROG TO REPEATED STIMULATION BY 15-PER-CENT ACETIC ACID

Experiment 1. Frog 3. March, 1932.

No. of trials	% of trials to end point	Reaction-time (sec.)	$1/RT$	Behavior
1	5.6	1.5	.666	Hit with left hind leg
2	11.1	1.6	.625	" " " " "
3	16.7	3.6	.278	" " " " "
4	22.2	2.0	.500	" " " " "
5	27.8	2.2	.454	" " " " "
6	33.3	7.3	.137	" " " " "
7	38.9	4.2	.238	" " " " " , jumped around before hitting
8	44.5	7.4	.135	" " " " "
9	50.0	7.2	.138	" " " " "
10	55.5	7.7	.130	" " " " "
11	61.1	10.5	.095	" " " " "
12	66.6	120.0	.017	No hit, restless, turning and moving
13	72.3	4.5	.222	Hit with left hind leg
14	77.8	8.0	.125	" " " " "
15	83.4	16.2	.077	" " " " "
16	88.9	21.0	.048	Missed twice with left front leg, hit with left hind leg
17	94.5	24.3	.041	Missed twice with left front leg, and right front leg, hit with left hind leg
18	100.0	31.7	.032	Hit with right hind leg
19	....	160.0	....	No hit, increasing restlessness
20	....	160.0	....	No hit, restless turning and moving

Circular pieces of filter paper, 4 mm. in diameter, were soaked in 15-per-cent acetic acid and placed on chosen dorsal cutaneous areas of normal intact frogs. The frogs, *Rana pipiens,* were placed one at a time in a glass jar and the selected area was repeatedly stimulated until no further response occurred. The first application of the acid invariably resulted in a vigorous and accurate kick of the foot nearest the spot, which dislodged the filter paper. If the area was immediately washed with a jet of Ringer's solution the frog became quiescent and generally remained so until restimulated. A procedure was used which allowed the frog a rest of one minute between washing with Ringer's solution and the next application of the acid-soaked filter paper to the same skin area.

With successive applications of the acid the localization was generally found to become increasingly less precise; on the eighth or tenth application the frog frequently required several strokes of the foot to remove the paper. As the experiment continued, ability to localize decreased qualitatively, the frog finally responding only by sidling or hopping away from the side stimulated without striking at the irritated spot. After some twenty trials no further response was usually obtained although the stimulation of an undamaged area gave the normal, immediate, localized reaction. This behavior was observed with some fifteen frogs.

An interesting feature of the behavior which was usually correlated with the increasing inability of the frog to localize was a lengthening of the reaction-time with successive stimulation. The reaction-time here refers to the time elapsing between the application of the acid and any negative response to the irritant, i.e., striking at the spot, sidling or hopping away from it.

Table 1 is a typical protocol, and Figure 1 is a plot of data from two experiments. Here the percentage of the number of applications of the stimulus until the failure of all response is plotted as abscissa against the reciprocals of the reaction-times. The reciprocals of reaction-times are used since they have the dimensions of rates and may be assumed to be directly proportional to the rate of the process or processes ultimately producing the response. Despite large variations in the data, the rate of the determinant of the response declines roughly hyperbolically with repeated stimulation.

There are a number of factors which may account for the variations of the reaction-times. In the first place, it was difficult always to place the filter paper on exactly the same area at each successive

repetition. Failure to do so would necessarily bring into action fresh receptors which might shorten the reaction-time unduly. Secondly, there was no very satisfactory way of controlling the exact amount of acid brought to the frog on successive stimulations. Moreover, varying areas of the skin, depending upon its degree of wetness, might be affected by diffusion of the acid from the region of the filter paper. That diffusion occurred was suggested by the fact that when India ink was mixed with the acid considerable spread was noted outside of the area covered by the filter paper.

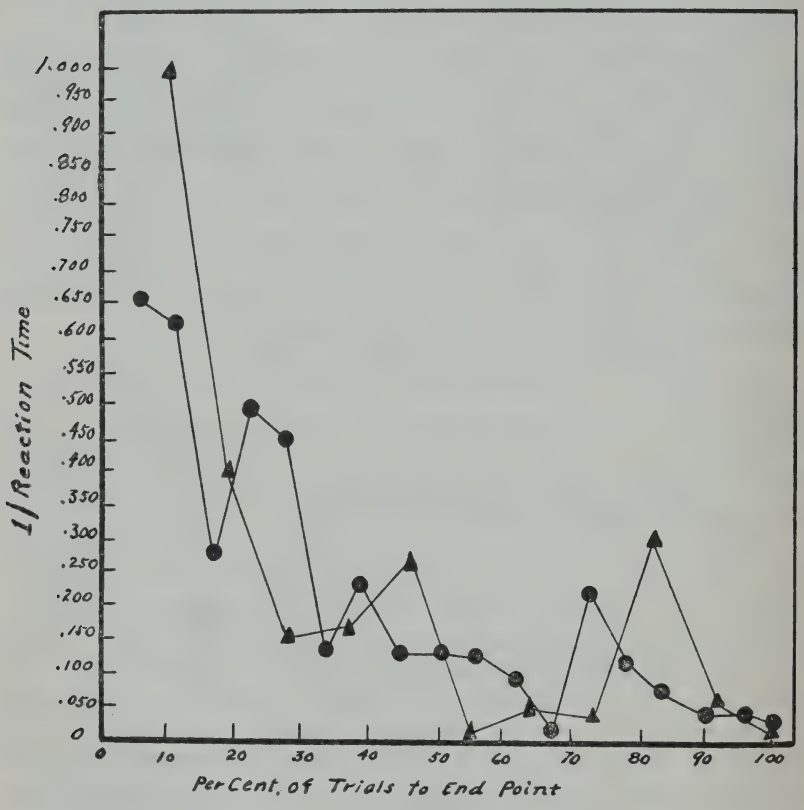

FIGURE 1

PLOT OF DATA OF TABLE 1 (SHADED CIRCLES)

The shaded triangles represent another similar experiment.

To avoid some of these difficulties a group of frogs were etherized and all of the dorsal cutaneous nerves but one were cut. The skin was then sewed together and the animals were allowed to recover. In this way the overlap of areas from dorsal cutaneous nerve trunks was eliminated although not, of course, the overlap of the areas supplied by the single fibers of the remaining nerve. One could stimulate the dorsal area supplied by the nerve without the danger of the acid spreading to other dorsal cutaneous nerve areas, although there was still the possibility of its spreading to areas supplied by lateral rami. To assure complete stimulation of the sensitive area the acid was brushed quickly over it, the approximate extent being known from previous studies (Hoagland, 4).

Table 2 is a typical record of an experiment with one of the operated frogs. The localized reaction still occurred but in most of these experiments the initial reaction-times are considerably longer than found with unoperated frogs, and the number of reactions which occur before failure of the response is reduced from an average of 15 to only 3 or 4. The failure of localization was similar to that of the unoperated animal.

The responses were found to be unaffected by removal of the forebrain and optic lobes, indicating localization in frogs to be independent of higher centers. Table 3 is a protocol of the responses of a decerebrate frog to acid stimulation.

III

The increasing durations of reaction-times with repeated stimulation as indicated in the tables were quite unexpected. Records of nerve impulses from a dorsal cutaneous nerve in response to acid stimulation show always a maximum frequency of discharge during

TABLE 2

RESPONSE OF A FROG TO REPEATED STIMULATION BY 15-PER-CENT ACETIC ACID
Only one nerve left intact
Experiment *2a*. Frog 1. February, 1932.

No. of trials	Reaction-time (sec.)	Behavior
1	2	Hit with left hind leg
2	10	Hit with left hind leg
3	59	Restless at 20 sec. Hit with left hind leg
4	120	Turned away from acid spot, occasional movements

1st Application
Stimulus
Impulses after
3.0 Secs.
2nd Application
Stimulus
Impulses after
5.8 Secs.

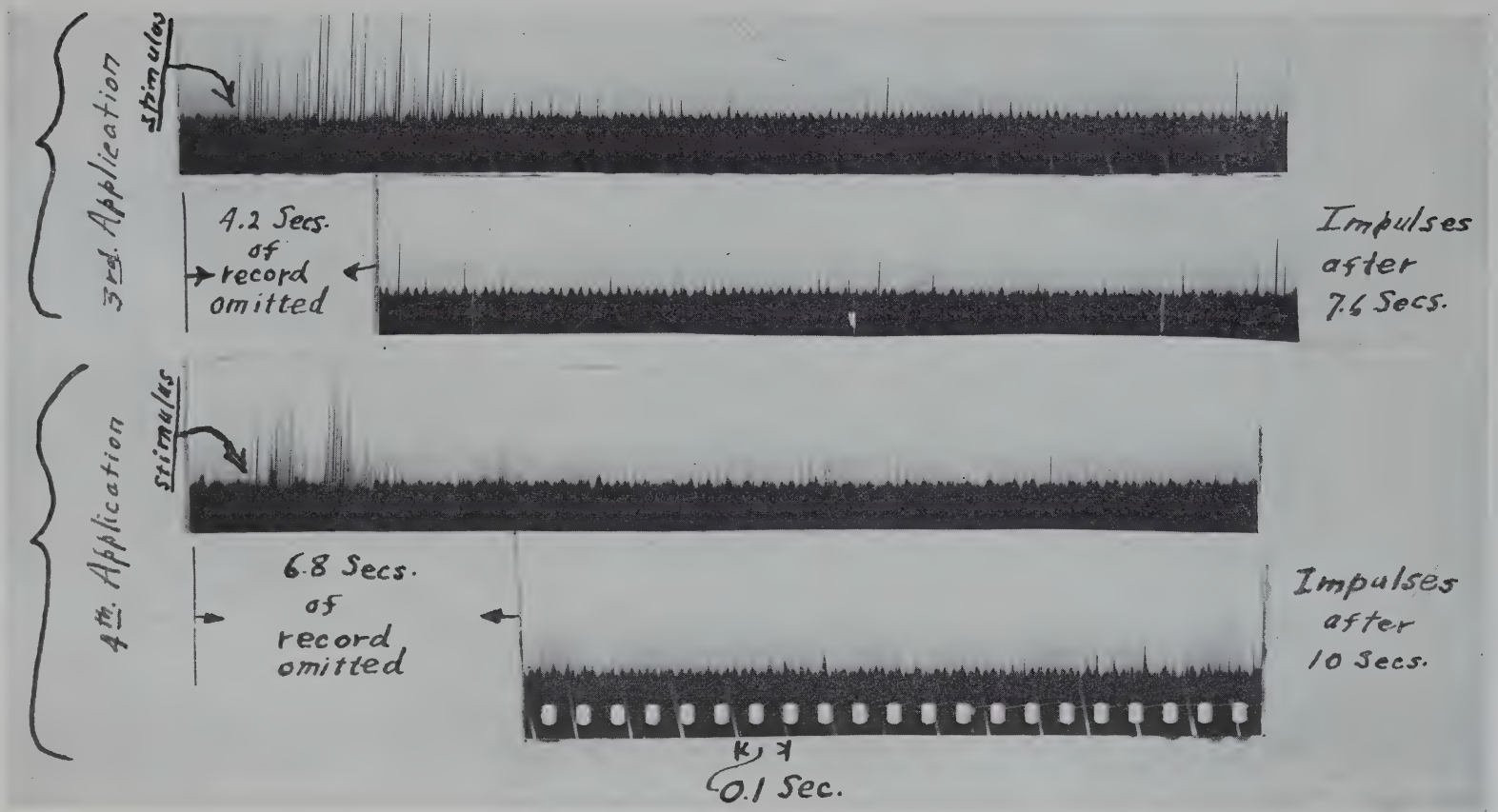

FIGURE 2

RECORDS OF NERVE IMPULSES FROM A DORSAL CUTANEOUS NERVE WHEN 15-PER-CENT ACETIC ACID WAS APPLIED SUCCESSIVELY AT ONE-MINUTE INTERVALS (SEE TEXT) TO THE AREA SUPPLIED BY THE NERVE

The intervals of 3.0, 5.8, 7.6, and 10 seconds correspond to the reaction-times previously obtained from the intact frog when an area on the side opposite to that used subs equently for recording purposes was repeatedly stimulated according to the method described in the text.

TABLE 3

RESPONSE OF A FROG TO REPEATED STIMULATION BY 15-PER-CENT ACETIC ACID

Only one nerve left intact—decerebrate frog

Experiment *2b*. Frog 8. January, 1933.

No. of trials	Reaction-time (sec.)	Behavior
1	.4	Hit with right hind leg
2	2.6	Hit with right hind leg, leaped at 2 secs. before localizing
3	13.2	Jumped at 1 sec., jumped at 7.8, missed at 7 sec., hit with right hind leg at 13.2 sec.
4	120.0	Moved vigorously at odd times, no hit
5	120.0	Moved away from spot, no hit
6	120.0	Moved away from spot, leaped forward, no hit

the first second of application of the acid, the frequency then declining progressively for a minute or more. If massiveness of discharge were correlated with "pain," one would expect the negative reaction to the stimulus, even after destruction of most of the endings, to occur, if at all, immediately after application of the acid.

Nerve impulses were recorded from dorsal cutaneous areas of frogs with spinal cords pithed by using the same technique of stimulation as used with intact frogs. The stimulus was applied and the impulses were photographed during a period of time corresponding to the first reaction-time recorded for the intact frog (Table 2) when a nerve on the opposite side to that subsequently operated on was stimulated. The skin of the pithed frog was washed with a jet of Ringer's solution and after a minute the stimulus was reapplied for a duration corresponding to the duration of the second reaction-time obtained from the frog before operating on it. This procedure of repeated exposures at increasing durations was continued until a point was reached at which no further response had been elicited from the previously intact frog.

Figure 2 is an illustration of impulses from a dorsal cutaneous nerve taken according to this procedure. Application of the stimulus is signalled by a burst of high-potential, rapid impulses of the A-type of Erlanger and Gasser (3). After the application, while the acid remains on the skin, some of these impulses continue, along with a discharge of slower impulses of smaller potential, B- or C-type impulses of Erlanger and Gasser. The speed of the impulse is inversely proportional to the thickness of the potential spike on

the record; the magnitude of the potential is indicated by the height of the spike. [For discussions of impulses from the frog's skin, see Adrian (1) and Hoagland (4).] With successive stimulations the A-type impulses become less in evidence due, presumably, to the more rapid destruction by the acid of the receptors initiating these impulses. The B- or C-type impulses continue over the nerve at a rather low frequency after the complete adaptation or permanent failure of the A-type response (Figure 2).

The response of the frog to the stimulus does not appear to depend upon the density of the discharge of nerve impulses but rather upon the summation of the effect of B- or C-type impulses, since after several repetitions of the stimulus the poorly localized avoidance reaction to the acid evidently occurs after the complete failure of A-type impulses, and after an interval of time sufficient to allow for the sum total of B- or C-type impulses, reached after perhaps a minute's discharge, to equal that produced in a few seconds of discharge when the skin is stimulated for the first time. The records suggest that the negative reaction to the stimulus depends upon a central summation of the effects of B- or C-type impulses, while specific localization depends upon A-type impulses occurring either in tactile fibers, which may also respond to acid, or in those few fibers which have been found to give A-type impulses but which respond only to chemical stimuli (cf. Hoagland, 4).

IV

To test further this hypothesis nerve impulses were recorded from frogs simultaneously with their gross behavior. The skin of an animal's side was anaesthetized with alcohol to prevent stimulation by acid of receptors of the lateral cutaneous nerves. The dorsal cutaneous nerves were exposed by a median incision and all of them were cut except one. The frog was placed in a dish and generally remained quiet until stimulated by the acid. Hooked electrodes, bent so as to prevent breaking the nerve, were then slipped under the remaining dorsal cutaneous nerve. Both decerebrate frogs and those with an intact central nervous system were used. The behavior in response to stimulation was essentially the same in both groups, but the former group was more quiescent between stimulations and was easier to work with.

After mapping the area supplied by the remaining cutaneous nerve, 15-per-cent acetic acid was brushed on the skin and photo-

graphs were taken just before, during, and after the stimulation. At the first application of acid the frog generally leaped within one to two seconds, very seldom localizing and striking at the spot with its foot. After washing the skin and after allowing the passage of the usual minute's rest interval the stimulus was repeated. This procedure was continued usually four or five times until no further response to the acid occurred. In some five frogs examined the reaction-times increased in a rough general way after the manner indicated in Tables 2 and 3, but the variation in reaction-time was considerable, and the results can be considered only as indicating a tendency of increasing reaction-times with repetitions.

The records of impulses, which we are sure were the only sensory impulses initiating the response, were similar to those shown in Figure 2 and confirm at least qualitatively the hypothesis that a summation of the effects of B- and C-type impulses is involved in the final response. Failure of localization in these preparations may have been the result of the lack of an overlap of areas supplied by different nerve trunks, although this hardly seems probable since, as we pointed out, animals in which all dorsal cutaneous nerves but one have been cut do localize. It is possible that there is sufficient overlap of areas supplied by lateral cutaneous nerves to account for localization in the earlier experiments in which the skin of the side was not anaesthetized. On the other hand, there is certainly considerable overlap of areas supplied by single nerve fibers within the area supplied by a nerve trunk (Hoagland, 4, 5).

Summary

1. Progressive chemical destruction of sensory endings in frog's skin produces a corresponding failure of the ability of the animal to localize to irritants (15-per-cent acetic acid) placed on the skin.

2. Rapid A-type impulses of high potential in cutaneous nerves are apparently necessary in order to elicit specifically localized responses of frogs to cutaneous irritants. Only crude localization is shown when impulses are of the B- and C-types.

3. The time elapsing between the application of a chemical irritant, which slowly destroys sensory endings, and the reaction to the irritant increases with successive applications of the stimulus.

4. An unspecifically localized avoidance reaction occurs after several applications of the irritant stimulus and in the absence of all A-type impulses. The reaction does not depend upon the *fre-*

quency of discharge of the nerve impulses, but rather appears to be determined by a threshold *total number* of impulses of the B- or C-type which have been set up in the time between the application of the stimulus and the response.

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SUR LA BASE NEUROLOGIQUE DE LA RÉPONSE LOCALISÉE AUX IRRITANTS SUR LA PEAU

(Résumé)

1. La destruction chimique progressive des extrémités sensorielles dans la peau de la grenouille produit une perte correspondante de l'habileté de l'animal dans la localisation aux irritants (acide acétique de 15%) placés sur la peau.

2. Les impulsions du type A rapide de haut potentiel dans les nerfs cutanés sont nécessaires, parait-il, pour causer les réponses spécifiquement localisées des grenouilles aux irritants cutanés. Seulement une localisation grossière se montre quand les impulsions sont des types B et C.

3. Le temps qui s'écoule entre l'application d'un irritant chimique, qui détruit lentement les extrémités sensorielles, et la réaction à l'irritant s'accroît avec les applications successives du stimulus.

4. Une réaction d'evitement non spécifiquement localisée se montre après plusieurs applications du stimulus irritant et en l'absence de toutes les impulsions du type A. La réaction ne dépend pas de la *fréquence* de la décharge des impulsions nerveuses, mais semble plutôt être déterminée par un *nombre total* du seuil des impulsions des types B ou C lesquelles se sont montrées dans le temps entre l'application du stimulus et la réponse.

Hoagland et Berkovich

ÜBER DIE NEUROLOGISCHE GRUNDLAGE LOKALISIERTER REAKTION AUF HAUTREIZUNGEN

(Referat)

1. Progressive chemische Zerstörung der Nervenenden in Froschhaut bewirkt ein entsprechendes Versagen der Fähigkeit des Tieres Reizmittel (15% Essigsäure) auf der Haut zu lokalisieren.

2. Schnelle, hochpotentielle A-Typ Reize in Hautnerven sind offenbar notwendig, um besonders lokalisierte Reaktionen des Frosches auf Hautreizmittel hervorzurufen. Nur grobe Lokalisation ist möglich, wenn die Reize den B- und C-Typen angehören.

3. Die Zeit, die von der Anwendung des chemischen Reizmittels, das die sensorischen Endungen langsam zerstört, bis zur Reaktion auf das Reizmittel wird durch aufeinanderfolgende Anwendung des Reizes verlängert.

4. Eine nicht speziell lokalisierte Ausweichereaktion tritt nach mehreren Anwendungen des Reizes und bei Abwesenheit aller A-Typ-Reize ein. Die Reaktion hängt nicht von der *Anzahl* der Reizungen pro Zeiteinheit, ab sondern scheint bestimmt zu sein durch eine *Gesamtzahlschwelle* der B- und C-Typ Reize, die der Zeit von der Anwendung des Reizes bis zur Reaktion hervorgebracht wurden.

HOAGLUND UND BERKOVICH

SIMILARITY OF MEANING AS A FACTOR IN RETROACTIVE INHIBITION*

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PROBLEM AND HISTORICAL BACKGROUND

The purpose of this study was to investigate the effect of varying degrees of similarity of meaning on retroactive inhibition.

Müller and Pilzecker (7), in 1900, first used the term "retroactive inhibition" to designate the effect upon retention of any work interpolated between learning and recall. Since that time many investigations have been conducted in an effort to determine the causes of this phenomenon and the conditions under which retention is most susceptible to such inhibitory effects.

DeCamp (2), in 1914, first suggested that similarity of the interpolated work to the original might be a determining factor. This hypothesis was drawn from a comparison of his rather negative results, when dissimilar material was used, with the findings of previous experimenters.

A rather extended study of the various factors involved in determining the degree of retroactive inhibition was made by Robinson (8) in 1920. For original learning he used series of eight four-place numbers. During the interval before retention was measured, his subjects learned a second series of four-place numbers, memorized series of twenty consonants, learned poetry, multiplied four-place by four-place numbers, or read the newspaper. He concluded that the degree of retroaction was a function of the similarity between the interpolated activity and the original learning. He also observed that this similarity may not be merely a one-dimensional affair but may be due to similarity of contents, of forms of presentation, or

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of the processes involved. On the basis of these conclusions, he formulated the following principle:

> "As similarity between interpolation and original memorization is reduced from near identity, retention falls away to a minimum and then rises again, but with decreasing similarity it never reaches the level obtaining with maximum similarity" (9, pp. 298-299).

Robinson (9) attempted to verify experimentally this theoretical curve by varying systematically the similarity of the interpolated work. He used the memory span method with a series of eight consonants. The last four were considered the interpolated material, and retention was measured in terms of correct written reproduction of the first four in the series. The degree of similarity varied from a condition of none in common to a condition in which all four of the second group were the same as the first four. The results when plotted show only the first phase of the theoretical curve with no inversion, that is, as similarity decreased retroactive inhibition increased.

Skaggs (11) has corroborated Robinson's work. However, his theoretical curve is in terms of two opposing functions, namely, reinforcement and inhibition. At the point of identity inhibition is at a minimum and reinforcement at a maximum. This situation is just reversed (that is, greatest inhibition and least reinforcement) when materials have a certain degree of similarity though are nonidentical. Finally, as original and interpolated work become more dissimilar the curve rises, due to inhibition decreasing, though not to the original minimum. He has recently made the following formal statement of the law as he conceives it:

> "Other factors being constant, there is a certain admixture of like and unlike elements of method or content which will result in a maximum amount of retroaction. With a decrease in the common elements retroaction will decrease, finally reaching a minimum. With an increase in common elements retroaction will decrease, reaching zero with identical elements" (12, p. 29).

Harden (3), in 1929, pointed out some interesting facts concerning previous experimentation and their relation to the principle of similarity. Both Robinson and Skaggs had used the term "similarity" to indicate the *same type* of material and "dissimilarity" to indicate a *different type* of material. According to this terminology Robinson's first study tested only the second phase of the curve he postu-

lated. Furthermore, she pointed out the impossibility of measuring quantitatively the similarity between four-place numbers, digits, and poetry. Robinson's work in 1927 included only the first phase of the curve, that is, from complete identity to non-identity but similarity (same type of material—consonants).

Harden supplemented the latter work of Robinson by using varying degrees of dissimilarity. The memory span method was used with two types of material, consonants and digits, and five degrees of similarity between the original and interpolated lists, that is, from none to four consonants in the last four of the series of eight. The results show recall more efficient under the most similar condition. When these results were combined with Robinson's data, the curve corresponded roughly to the theoretical one except that the latter phase was concave rather than convex.

Cheng (1) studied the retroactive effects of similarity by using, for original and interpolated materials, lists of nonsense syllables that exhibited varying degrees of similarity in respect to spelling. His results varied with the method of measurement used. By recall scores, the retroactive effect at first increased and then decreased as the degree of similarity increased from zero to almost identity. By the saving method, the retroactive effect exhibited a slight tendency to decrease with the increase of similarity, although this trend was not marked or consistent.

The work thus far cited has dealt chiefly with non-meaningful materials. There have been also two experiments reported in the realm of meaningful relations.

Whitely (13) studied the effect upon recall of intellectual activities at varying intervals prior to recall. The activities introduced were congruous and non-congruous with the original learning. Meaningful materials were used and recall was by the association test method after 48- and 96-hour periods. He found that "the arousal prior to recall of an apperceptive disposition which is closely related to (congruous with) the material memorized has a greater inhibitive effect upon that recall than the arousal of an apperceptive system which is less closely related to (non-congruous with) the memorial material" (13, p. 506).

McGeoch and McDonald (6), in 1931, studied the similarity factor in retroaction with meaningful relations. They used a list of 11 adjectives for original learning, and synonyms, antonyms, unrelated adjectives, nonsense syllables, and numbers for interpolated

material. The original list was given five presentations, the interpolated list 10, and then learning was continued to the point of three perfect repetitions of the first list. In the rest condition the subject read a humorous magazine or engaged in conversation with the experimenter for a period of 10 minutes. They found that retroactive inhibition varied directly as the degree of synonymy between the original and interpolated learning.

In order to interpret properly the effect of similarity in determining retroactive inhibition, it is necessary to understand the part played by other factors in the experiment.

Robinson and Heron (10) studied the relationship between the length of memorized material and its susceptibility to retroaction. They concluded that susceptibility to retroactive inhibition decreases with length after a certain minimum length has been passed. The lists used in their experiment were 6, 9, 12, 15, and 18 syllables. The interpolated list was 12 syllables.

McGeoch (4) has shown that the retroactive effects vary indirectly with the degree of learning of the original material. Also, in a study as yet unpublished (referred to in 5), he has found that the time spent on interpolated work is also a determining factor.

Various theories have been proposed to explain the phenomenon of retroactive inhibition. Müller and Pilzecker (7) interpreted their results in terms of a perseveration theory. Skaggs (12) favors this hypothesis in the main. McGeoch (4, 5, 6), however, has rejected this explanation and attempted to prove by his experimental work and theoretical discussions that a theory of transfer is a better explanation of the phenomenon. Robinson (8, 9), also, interpreted his results as supporting the transfer theory.

The present study is concerned with the degree of retroactive inhibition produced by varying degrees of similarity of meaning between the original and interpolated learning under conditions somewhat different from those of previous studies.

Procedure

Material. The material used consisted of lists of 21 abstract nouns. The interpolated lists varied in degrees of similarity of meaning to the original lists.

A list of abstract nouns with their synonyms was taken from *Webster's Unabridged Dictionary*. Copies of this list were given to judges (graduate students) who were asked to rank the synonyms

according to their relative degree of similarity of meaning to the given noun. On the basis of these judgments, words were selected for this experiment that were given the same ranking by fifteen judges.

Material for Condition A consisted of two lists. The interpolated list was composed of those synonyms that were given rank *1* by the 15 judges, that is, the word most similar to the corresponding word in the list used for the original learning. In Condition B the interpolated list was made up of words given rank *2* by the judges. Likewise, in Condition C the interpolated list was selected from those that were ranked *3*. In all three of the work conditions the pair of synonyms occupied the same position in the two lists. Condition D was the control condition in which no interpolated learning was introduced. The material used was a miscellaneous list of 21 abstract nouns.

In compiling the various lists certain precautions were taken in order to make the lists as nearly as possible of equal difficulty. For example, the order of the words in the lists was such that no two words appeared in succession that began with the same letter or prefix or ended with the same suffix. No hyphenated words were used, and all words were longer than three letters. No word was used that has the same form for both noun and verb. This was to prevent the possibility of making a subject-predicate relation in learning the list. No word was repeated in any of the lists.

Subjects. Twenty-nine adults served as subjects, 4 undergraduate students and 25 graduates. Each subject was given all four conditions of the experiment.

Apparatus. An electrically revolved memory drum was used which was timed for a 2-second exposure with a 10-second interval between the presentations of the list. The words for each condition were typed in capital letters on a belt made of drafting linen. In the case of the work conditions the shift could be made to the second list immediately.

The experimenter was seated behind a screen to the left of the subject. By means of duplicate lists of words, a record was kept of all responses. A stop-watch was used to keep the time interval constant between learning and recall.

Method. The lists were learned by the anticipation method with oral recall. The first word in each list served as a cue so that 20 connections had to be made. One correct anticipation was the

criterion for learning. Retention was measured by the recall, relearning, and saving score methods.

In all conditions the first list was recalled and relearned 20 minutes after the end of the learning. In Conditions A, B, and C, the interpolated list of synonyms was learned during this interval. Any time remaining before the end of the 20-minute period was spent reading the newspaper or engaging in conversation with the experimenter. In a few instances the subject failed to learn the interpolated list in the allotted time. In such cases the first list was recalled and relearned as soon as the second was learned to one correct repetition. In Condition D, the control condition, the subjects either read the newspaper or conversed with the experimenter during the entire 20-minute period. An effort was made to let the subject rest and at the same time prevent any rehearsal of the lists of words.

The subjects took the conditions in all possible orders to eliminate practice effects.

Results

Table 1 gives the means, standard deviations, and probable errors of the original learning trials, interpolated learning trials, number of words correctly recalled on the first relearning presentation, and the relearning trials.

TABLE 1
Averages

Condition	A	B	C	D
Trs. to learn				
Mean	22.2	19.9	19.1	26.5
Sigma	6.95	8.36	6.49	11.74
P.E.	.86	1.04	.81	1.46
Interpolated trs.				
Mean	15.6	16.3	16.9	
Sigma	4.06	6.58	7.41	
P.E.	.50	.82	.92	
No. recalled				
Mean	6.9	8.5	10.1	15.1
Sigma	4.65	5.19	4.81	3.6
P.E.	.58	.65	.60	.45
Relearning trs.				
Mean	9.7	7.7	5.4	6.1
Sigma	4.41	4.42	2.67	4.23
P.E.	.55	.55	.33	.53

The average number of trials to learn the original lists in the three work conditions varies only slightly. In absolute amounts they rank in descending order from Condition A to C. However, the average number of learning trials spent on Condition D, the control condition, is greater than that of any of the other three. Inspection of the individual records shows that the subjects who took this condition first demonstrated the greatest practice effect.

The differences in the average number of trials spent in learning the interpolated lists are so small as to be negligible. In absolute amounts they rank in the order C, B, A, from the largest to the smallest.

The average number of relearning trials descends steadily in the three work conditions from A to C. However, the number of trials to relearn in Condition D is greater than in Condition C, although the difference is very slight. This inversion corresponds to the greater number of learning trials in the same condition. The reliability of the differences between the relearning trials of the various conditions is given in Table 2.

In terms of saving scores the control condition (D) was the most favorable for retention (Table 3). When these scores are plotted, the curve rises from A to D indicating a gradual rise in retention as the similarity of the interpolated and original learning decreased.

The relative amounts of retroactive inhibition produced in the three work conditions were obtained by using the rest condition as a basis (Table 4). The curve of retroaction drops from 26.9% when interpolation was most similar to 20.4% with the second degree of

TABLE 2

Comparisons between Mean Recall and Relearning Scores of the Various Conditions

Conditions compared	Recall			Relearning		
	Diff.	P.E.$_d$	Diff. / P.E.$_d$	Diff.	P.E.$_d$	Diff. / P.E.$_d$
D—A	8.2	.73	11.23	3.6	.76	4.74
D—B	6.6	.79	8.35	1.6	.76	2.11
D—C	5.0	.75	6.67	—0.7	.62	1.13
C—A	3.2	.83	3.86	4.3	.64	6.72
C—B	1.6	.88	1.82	2.3	.64	3.59
B—A	1.6	.87	1.84	2.0	.78	2.56

TABLE 3
AVERAGE SAVING SCORES AND PERCENTAGES OF RECALL

Condition	Saving score	Percentage recalled
A	56.3	34.5
B	61.3	42.5
C	71.7	50.5
D	77.0	75.5

similarity, and finally reaches the lowest point at 6.9% when the interpolated learning was least synonymous to the original (Figure 1).

The retroactive effect is very much more marked when retention is considered in terms of recall scores (Table 3). The rest condition gives by far the greatest number of words correctly recalled on the

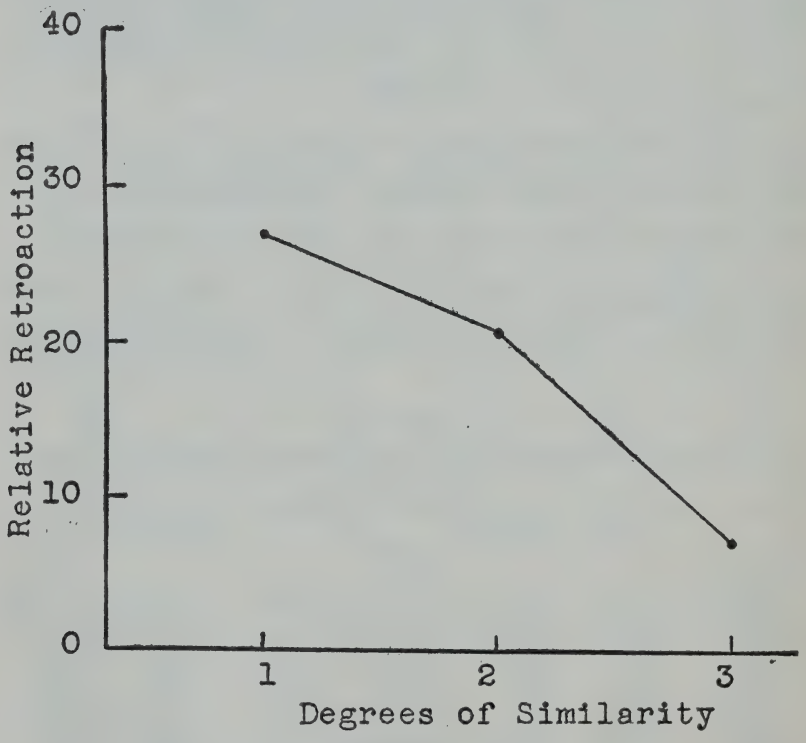

FIGURE 1
RELATIVE RETROACTIVE INHIBITION (SAVING)

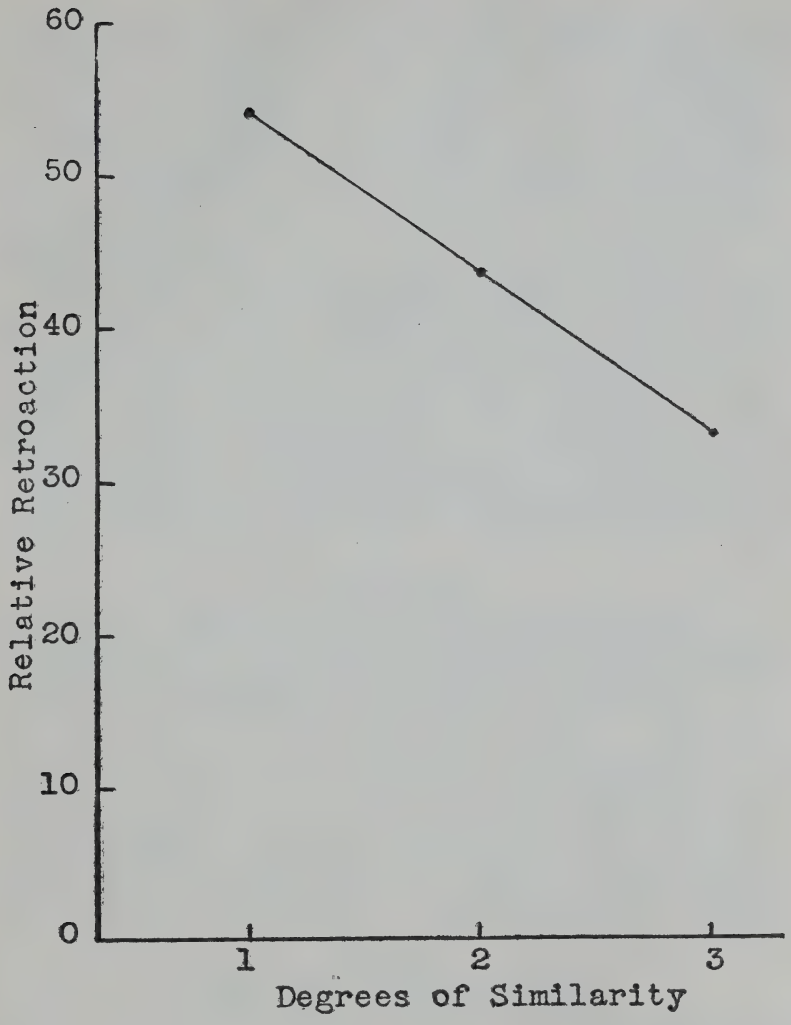

FIGURE 2
RELATIVE RETROACTIVE INHIBITION (RECALL)

TABLE 4
RELATIVE RETROACTIVE INHIBITION (SAVING)

Work conditions	A	B	C
Rest (D)	.770	.770	.770
Work	.563	.613	.717
Diff.	.207	.157	.053
Diff./Rest	.269	.204	.069

first relearning presentation (Table 1). The differences between the rest condition and all the other conditions are shown to be reliable in Table 2. Also, the difference between the recall scores in the least similar (C) and the most similar condition (A) is 3.2 with a P.E. of the difference of ±.83. The curve of retention rises from A to D as in the case of the saving scores. When the relative retroaction is figured on the basis of the percentage recalled in the rest condition (Table 5), the curve is almost linear (Figure 2). The retroactive inhibition decreases with the similarity of the interpolation from 54.3% in A to 43.7% in B, and even in the least similar condition it is as great as 33.1%.

CONCLUSIONS

1. Retention was more efficient after rest than after interpolated learning by recall, relearning, and saving scores with the exception of one relearning score. In this case the difference is statistically unreliable.
2. Retroactive effects were greater when measured by the recall than by relearning and saving scores.
3. The general trend by all criteria was for retroactive inhibition to decrease as the similarity decreased between the original and interpolated learning. On the basis of this fact, similarity of meaning is judged to be a determining factor in retroaction in this experiment.

TABLE 5
RELATIVE RETROACTIVE INHIBITION (RECALL)

Work conditions	A	B	C
Rest (D)	15.1	15.1	15.1
Work	6.9	8.5	10.1
Diff.	8.2	6.6	5.0
Diff./Rest	.543	.437	.331

Discussion

In the course of the experiment many interesting observations and comments were made by the subjects. Some noticed the similarity between the lists more than others. However, it is interesting to note that those subjects who insisted that they learned the words by rote without making any meaningful connections would occasionally give a synonymous word from the interpolated list while relearning or give a synonym not in either of the lists. Other subjects noticed the similarity and reported that they thought it was the cause of their difficulty in relearning.

Any effect the first list might have had on the second seems to have been beneficial since the average number of trials to learn the interpolated list was invariably less than the number necessary for original learning. This was almost universally true in case of the individual records as well. This may be partially due to a warming-up effect. The most similar interpolated list (A) was learned in fewer trials than the other two, and the B list was learned quicker than C although the differences are not great. Thus, the greater retroactive effects in Condition A could not be explained in terms of greater time spent on interpolated learning.

Neither can the retroaction be interpreted as due to less perfect learning since retention was least in the work condition that took the greatest number of trials to learn. The original learning trials for these conditions rank in the order A, B, C.

The conditions of this experiment differed from most others before it in that the lists were longer and learning was carried to completion on both the original and interpolated lists. In the other experiments already cited shorter lists were given a constant number of trials in most cases unless for the sake of comparison to discover the conditions most susceptible to retroaction. Within limits, the longer the list (10) and the greater the number of presentations (4), the less retroactive inhibition has been observed. When these facts are taken into consideration, the retroactive effects obtained in this study are more pronounced than they appear from the data alone.

These data may be interpreted in terms of a transfer theory in so far as retroaction was caused by a transfer of synonymous words from the interpolated list to the original in relearning.

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LA SIMILARITÉ DE SIGNIFICATION COMME FACTEUR DE L'INHIBITION RÉTROACTIVE

(Résumé)

On a fait une étude de l'effet de divers degrés de similarité de signification sur l'inhibition rétroactive. Le matériel s'est composé de listes de vingt-et-un noms abstraits. Les listes intercalées ont montré trois degrés de similarité de signification aux premières listes selon l'évaluation de trois juges. Les paires de synonymes ont occupé les mêmes positions dans la première liste et les listes intercalées. Pour faciliter la comparaison, on n'a rien intercalé dans une condition. On a appris les listes jusqu'à une répétition parfaite au moyen de la méthode d'anticipation avec rappel oral. On a mesuré la rétention au moyen des méthodes de rappel, de réapprentissage, et d'épargne vingt minutes après la fin de l'apprentissage. Vingt-neuf adultes ont subi toutes les conditions de l'expérience. Selon tous les critéres, l'inhibition rétroactive a tendu en général à décroître comme la similarité a décru entre le premier apprentissage et l'apprentissage intercalé. Les effets rétroactifs ont été plus grands quand on les a mesurés par le rappel que quand on s'est servi du réapprentissage et de l'épargne. Avec le rappel, la rétroaction relative a été de 54,3%, de 43,7%, et de 33,1% pour le premier degré, le deuxième degré, et le troisième degré de similarité, respectivement.

Johnson

DIE ÄNLICHKEIT DER BEDEUTUNG ALS BESTANDTEIL DER RÜCKWIRKENDEN HEMMUNG

(Referat)

Man untersuchte die Einwirkung von verschiedenen Graden der Änlichkeit der Bedeutung auf die rückwirkende Hemmung. Das Material bestand aus Listen von 21 abstrakten Hauptwörtern. Die interpolierten Listen zeigten drei Grade der Änlichkeit der Bedeutung mit den ursprünglichen Listen, den Rangordungen von 15 Richtern nach. Die Synonymenpaare hatten in den ursprünglichen und in den interpolierten Listen die selbe Stelle. Um Vergleichungen zu ermöglichen, wurde in einer Situation keine Interpolierung eingeführt. Jede der Listen wurde mit der Voraussagungsmethode [anticipation method], mit münlicher Wiederholung, gelernt bis sie einmal durch richtig wiederholt werden konnte. Die Retention wurde zwanzig Minuten nach Ende des Lernens mit den Methoden der Wiederholung [recall], des Wiederlernens [relearning] und des Ersparungsbetrags [saving score method] gemessen. Neun und zwanzig Erwachsene wurden allen Bedingungen der Untersuchung unterworfen. Allen Kriterien nach, erwies die rückwirkende Hemmung im Allgemeinen die Neigung, bei Abnahme der Änlichkeit zwischen dem ursprünglichen und dem interpolierten Lernstoff gleichmässig abzunehmen. Die Einwirkungen der Rückwirkung erwiesen sich, wenn sie mit der Wiederholungsmethode gemessen wurden, als grösser, als wenn sie mit der Methode des Wiederlernens oder mit der Ersparnismethode gemessen wurden. An den, mit der Wiederholungmethode erhaltenen, Zahlen gemessen, betrug die relative Rückwirkung, bei dem ersten, zweiten, und dritten Grad der Änlichkeit respektiv 54.3%, 43.7%, und 33.1%.

JOHNSON

THE EFFECT OF PRESSURE MAGNITUDE ON CUTANEOUS LOCALIZATION*[1]

From the Department of Psychology of The Ohio State Universtiy

DOROTHY ROSE DISHER

INTRODUCTION

Problem. A study of the localizing reactions to different pressures offers the possibility of both theoretical and technical contributions. Theoretically, it may contribute to our knowledge regarding the part played by the nerve supply and the importance of the "intensity pattern." Technically, it may offer a solution to the problem of the necessity of maintaining an absolutely constant pressure during cutaneous localization experiments.

History. Ponzo (2) in 1911 and Franz (1) in 1913 reported studies of this same problem. Ponzo found no difference in the accuracy of localization of the pressures he used. Franz, on the other hand, interprets his results as indicative of a difference. His method, however, was such that the experimental error could have been as large as the average variation of his results. His subjects were instructed to localize a given stimulated point with the end of the index finger. Assuming that the spot localized was at the geometrical center of the localized surface, he concluded that the light touch is more accurately localized than is heavy pressure. The results of these investigations are shown in Table 1.

Ponzo attributes the difference in error between the localization of 1.5 gr./mm. and 1 gr./mm. to an increase in attention of the subject necessary to perceive the stimulus distinctly.

PRESENT INVESTIGATION

Apparatus. The apparatus used in this experiment was designed for the purpose of the present investigation. It is illustrated in Figure

*Recommended by F. C. Dockeray, accepted for publication by Carl Murchison of the Editorial Board, and received in the Editorial Office, November 9, 1932.

[1]The writer wishes to express her appreciation of the assistance given her by Dr. Samuel Renshaw in laying out the program of the experiment and discussing the problem with her.

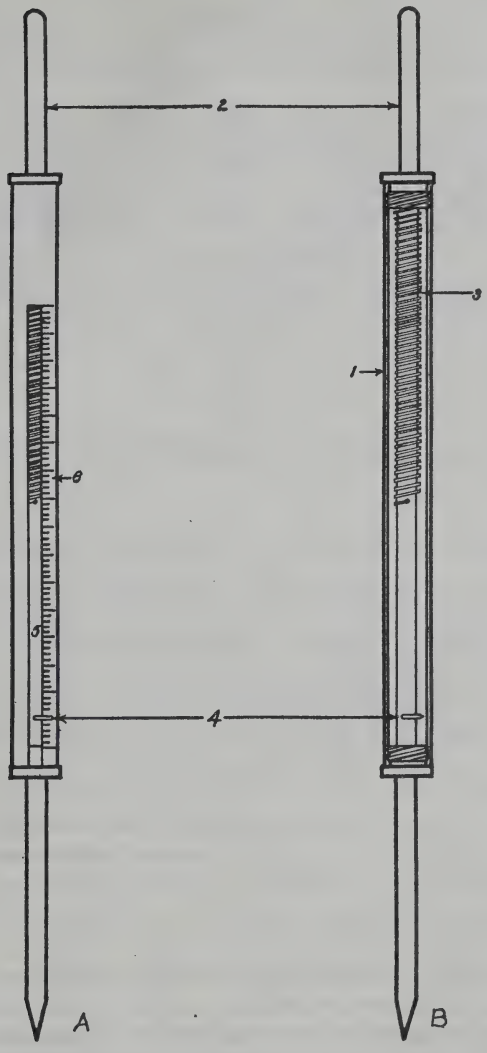

FIGURE 1

DIAGRAM OF THE SPRING AESTHESIOMETER DESIGNED TO GIVE GRADATIONS IN PRESSURE

Description appears in text.

A—external view
B—cross section

TABLE 1

FRANZ'S RESULTS

Subject	No. of trials	Area	Touch Error mm.	Touch M.D. mm.	Pressure Error mm.	Pressure M.D. mm.
C	30	back	16.0	7.2	30.3	11.0
A	20	abdomen	9.0	4.6	9.0	4.8
	20	upper leg	14.8	3.5	17.0	6.0

PONZO'S RESULTS

Stimulus gr./mm.*	Average error in mm.	Average variation in mm.
10	10.7	3.8
3	10.7	3.9
1.5	10.7	4.6
1	9.8	4.1

*Ponzo does not make clear the meaning of gr./mm. (See pp. 8 and 9 of his article.)

1. It consists of a bakelite barrel (1), 10.3 cms. long, through which is suspended a metal stylus with a ball point, .07 mm. in diameter (2), on a spring (3) that is attached to the metal cap of the upper end of the barrel. A pointer (4) fastened to the stylus and projecting through a perforation (5) in the barrel indicates the position of the stylus or the amount of compression of the spring. The instrument was so calibrated that the amount of pressure applied could be read from the position of the pointer on a centimeter scale (6) along the perforation. The apparatus was so constructed that spring supports (3) of various strengths could easily be inserted. The localizations were made with a stylus identical to the one used in the pressure apparatus. The stimulus is given in grams pressure, the weight necessary to balance it on the pan of a chemical balance.

In the application of the stimulus the experimenter compressed the spring until the pointer registered the desired amount of pressure. The stylus was then held in position by the pressure of the right forefinger against it above the barrel. The apparatus was then lowered to the spot where the pressure was to be applied and just as it touched the skin the stylus was released and immediately withdrawn. Some error was introduced here due to the variable responses of the experimenter in estimating the position of the pointer in relation to the skin. The time of application as measured mechani-

cally[2] was about 3/5 sec., and did not vary in direct relationship with the amount of pressure, so that any difference subsequently found could not be interpreted as due to difference in actual time of stimulation.

General Procedure. The subject used the Weber tactual-kinaesthetic method of localization. The experimenter stimulated a point, and the subject searched for it by the continuous method. That is, he moved his right hand from its resting-place to his left hand or arm and was allowed to make searching movements there until he was satisfied that he was over the right spot. The use of this method rather than the single movement where no searching is allowed complicates the time factor and makes the localization rather a series of localizations. Unpublished results which Dr. Renshaw has obtained in his laboratory indicate also that the errors are smaller with the use of this method as compared with the single-movement method. The subject kept his eyes closed during the experiment, located his spots, and made a judgment as to the accuracy of localization (in some cases). He was allowed to see his results at the end of the experimental period if he desired to do so.

The subject sat in approximately the same position during each experimental period. When he came for the experimental hour he assumed a comfortable position which he was required to maintain throughout the period. The position of the localizing hand in relation to the experimental area was also kept constant.

The back of the hand and the volar surface of the left forearm were chosen as the areas of investigation since they have been most commonly used in cutaneous localization experiments. An area 2 cm. square was marked off on the surface to be studied by means of a 2-cm. sectioned rubber stamp. After the ink had dried, each of the four corners was marked by the application of a sharpened point dipped in a 15% solution of silver nitrate. These points turned black and usually lasted throughout the experiment. That they did not serve as points of reference is indicated by the fact that the subjects would frequently move the stylus over them and no cues would arise from such stimulation. After the surface had been marked, the experimenter proceeded to locate pressure spots which

[2]The stimulus was applied, as during the experimental period to the skin, to the surface of a Marey tambour. The time of application and variation in pressure were recorded mechanically on a kymograph record simultaneously with a time line made by a Jacquet chronometer set at 1/5 sec.

were not complicated by either pain or cold spots.[3] The ink wore off or was washed off so that the area was kept in perfectly normal condition from day to day. The spots were re-located by restamping the area, that is, by placing each corner of the rubber stamp on the black point that indicated its position on the first day. The simple, uncomplicated pressure spots did not always remain simple. Whenever pain or pressure was reported in addition to pressure in a series of preliminary examinations on each day to ascertain this fact, a simple pressure spot in closest proximity to the one of the previous day was chosen for investigation. Unusually hairy areas were shaved for the experiment.

In determining the magnitude of error a sectioned square made by the rubber stamp was placed upon the record card, one for each spot to be investigated. The position of the spot on the arm was indicated on the square on the record card with reference to the different coordinates. The spot designated by the subject as the one stimulated was marked in a similar manner and numbered according to its position in the series of localizations, and the distance between these two points was measured by means of a millimeter rule. If the spot designated by the subject were off the square, direct measurement was made upon the arm. The measurement was made immediately after each localization and the number of the localization, the spot stimulated, the magnitude of the error, and the direction of the error were recorded in tabular form before the next stimulation was given. A new record was made for each subject for each day's results.

Series I. In the first series of experiments five different pressures were used, 12 gms., 27 gms., 40 gms., 57.5 gms., and 67.5 gms. Six pressure spots were located in the manner described above and each of these spots was stimulated once with each of the five pressures, thus making 30 localizations per day. The time for each localization and measurement of error averaged about 1.5 minutes. There was a regular serial order followed: Each of the spots, 1, 2, 3, 4, 5, and 6, was stimulated in that order by pressure No. 1, then in similar order by pressure No. 2, and so on through pressure No. 5. On the next day the order of spots stimulated was the same but this time the period opened with pressure No. 2 and so on with succeeding days through pressure No. 5, at which the procedure began over again.

[3]Warm was never a complicating factor.

Each person served as subject for the experiment on 10 different days, thus giving 300 localizations for each area, or 60 localizations for each pressure for each subject. In no case were the investigations made on consecutive days from the beginning of the experiment to the end. A regular procedure was followed, however, the briefest period from the first experimental day to the last being 12 days and the longest 21 days. The subject came at the same hour each day.

There were ten areas investigated, one on the back of each of five hands, and one on the volar surface of five left forearms. Eight subjects were used, two of whom served as subjects for both the back of the hand and the arm. The subjects[4] for the hand were (1) Dr. F. C. Dockeray, Professor of Psychology, (2) J. H. Taylor and (3) D. R. Disher, Assistants in Psychology, (4) K. Stewart, secretary, and (5) A. M. Bennett, undergraduate; those for the forearm were (6) Dr. W. L. Valentine, Professor of Psychology, (7) W. Schwartzbek, Graduate Assistant, (8) C. Hazard, graduate student, and (3) Disher, and (5) Bennett.

Results. The results of the experiment are given in Tables 2 and 3. The average magnitude of error, the σ of errors, and the critical ratios are given for each of the subjects, and also for the total number of localizations on the hand and on the arm. It will be remembered that Ponzo found no difference in the error of localizing, no matter what the pressure applied, and that Franz's results indicate a difference in favor of the lighter pressures generally, although this was not true for the abdomen. The present figures are consistent in one thing: in no case was the lightest pressure most accurately localized, thus opposing Franz's results. As far as the data for the back of the hand are concerned, there is no critical ratio that would indicate any significant difference between the localizations of the different pressures, the critical ratios ranging widely, from 0.02 (between 40 gms. and 67.5 gms. for Subject 4) to 2.00 (between 27 gms. and 40 gms. in case of Subject 3). For the combined results the ratios range from 0.17 (between 57.5 gms. and 67.5 gms.) to 1.41 (between 12 gms. and 57.5 gms.).

The data for the volar surface of the forearm are less easily interpreted. In one case (Subject 7, see Table 3) a statistically sig-

[4]The experimenter wishes to express her appreciation of the service rendered her by these subjects in giving freely of their time, and especially to thank Dr. Valentine for his advice during the experiment and A. M. Bennett who served as experimenter while she herself served as subject.

TABLE 2

AVERAGE ERRORS AND THEIR STANDARD DEVIATIONS GIVEN IN MILLIMETERS FOR LOCALIZATION OF DIFFERENT PRESSURES

Stim. value	12 gms. M.	12 gms. S.D.	27 gms. M.	27 gms. S.D.	40 gms. M.	40 gms. S.D.	57.5 gms. M.	57.5 gms. S.D.	67.5 gms. M.	67.5 gms. S.D.
				Back of left hand						
S. 1	7.06	3.9	7.04	3.6	6.91	4.5	7.31	3.8	7.66	4.4
S. 2	7.23	4.5	6.77	4.1	6.65	4.1	6.00	4.9	5.93	3.8
S. 3	4.04	2.7	3.91	2.3	4.75	2.4	4.43	2.9	3.87	2.5
S. 4	11.81	5.6	11.17	4.8	10.69	5.3	10.14	4.1	10.71	4.8
S. 5	5.89	2.8	5.16	3.3	5.28	3.2	5.59	3.0	5.61	3.1
Av.	7.21	4.7	6.93	4.6	6.86	4.4	6.69	4.3	6.76	4.4
				Volar surface of left forearm						
S. 3	5.16	2.7	5.50	3.8	5.11	4.1	4.96	3.4	4.63	3.0
S. 5	6.66	3.6	7.56	4.2	5.81	2.5	7.07	3.8	7.44	4.1
S. 6	8.54	4.8	8.09	4.4	6.66	3.8	7.31	4.1	6.76	3.7
S. 7	9.27	5.7	7.49	4.8	6.88	5.0	7.35	4.5	7.01	3.6
S. 8	6.30	3.1	7.00	4.1	6.22	3.4	7.07	4.3	7.09	4.0
Av.	7.19	4.5	7.13	4.5	6.14	3.7	6.75	4.1	6.59	3.8

TABLE 3

CRITICAL RATIOS SHOWING THE SIGNIFICANCE OF THE DIFFERENCE BETWEEN THE MEANS OF MAGNITUDE OF ERRORS AT DIFFERENT PRESSURES, COMPUTED FROM S.D.'S

Pressures compared	12–27	12–40	12–57.5	12–67.5	27–40	27–57.5	27–67.5	40–57.5	40–67.5	57.5–67.5
				Back of left hand						
Sub. 1	.03	.19	.35	.78	.17	.40	.86	.52	.92	.47
Sub. 2	.45	.73	1.54	1.68	.16	.93	1.16	.80	.98	.08
Sub. 3	.29	1.54	.76	.36	2.00	1.08	.09	.72	1.05	1.12
Sub. 4	.04	1.13	1.52	1.17	1.17	1.61	1.20	.33	.02	.35
Sub. 5	1.30	1.20	.50	.50	.20	.70	.70	.50	.60	.03
Total	.89	.94	1.41	1.18	.19	.65	.65	.47	.25	.17
				Volar surface of left forearm						
Sub. 3	.43	.08	.35	1.02	.54	.82	1.40	.21	.77	.57
Sub. 5	1.40	1.50	.62	1.10	2.70	.68	.16	2.60	2.58	.51
Sub. 6	.54	2.51	1.52	2.31	1.90	1.09	1.98	.91	.22	.51
Sub. 7	2.32	2.88	2.52	3.11	.68	.16	.62	.54	.16	.46
Sub. 8	1.07	.13	1.11	1.21	1.14	.09	.12	1.19	1.30	.02
Total	.16	3.06	1.24	1.76	2.91	1.06	1.58	1.90	1.45	.50

nificant difference between errors for 12 gms. and 67.5 gms. appeared. C.R.'s between 12 gms. and all other pressures, 2.32, 2.88, and 2.52, were likewise higher than the average. Reference to Table 2 which presents the magnitude of error shows that for this subject the least accurately localized pressures were the light ones, quite contrary to Franz's general findings. This difference may not be actually significant, however, but rather due to some half-dozen very large errors which may have been really accidental. The only other individual C.R.'s that even approach significance are those of Subject 5 for 27 gms. and 40 gms., and 40 gms. and 67.5 gms., and 2.7 and 2.58 gms., respectively, the most accurate localizations being made in these cases to the medium pressure of 40 gms. When the data are treated as a whole we find that there is a consistent and statistically significant tendency for more accurate localizations in favor of the 40-gm. pressure as compared with 12-gm. and 27-gm. pressures, the C.R.'s being 3.06 and 2.91. The average for the errors for localizing the 40-gm. stimulus (see Table 2) was 6.14 mm., that for the 12-gm. stimulus 7.19, and that for the 27-gm. stimulus 7.13 mm. The actual difference is about 1.0 mm. By referring to Ponzo's results, page 392, it will be seen that he has almost as great a difference between the localization errors with stimuli of different magnitudes, but he considers it non-significant. Not enough data are given to demonstrate the statistical reliability of the difference, but if it should prove reliable statistically it would quite contradict our results, indicating that the lighter pressures are more accurately localized than the heavy ones. With respect to our own data it is possible that the statistically real difference is in reality a pseudo-real difference. The difference between the means under consideration is only 1.05 mm., which proves upon inspection to be a small extent on a surface the size either of the back of the hand or of the forearm. The relationship of the means of magnitude of error is shown in Figure 2. It must be remembered, however, that this graph is a highly magnified representation of the actual condition and that our inability to define the area of tactual stimulation is very likely to be as great as this error. The C.R. of 3.06 cannot be taken under these circumstances as demonstrating a true difference. The error for the remaining pressures are not differentiated from those of either 12 gms. or 40 gms. when subjected to statistical treatment. *The comparison of light-pressure localizations with heavy-pressure localizations does not give any significant difference* (within the limits of this experiment).

Series II. It might be argued, however, that the variation in pressure used might not have been great enough to demonstrate a difference that really does exist. Accordingly a second series of experiments was conducted.

Procedure. Two pressures were used, one, "T," so light as to be only touch and not registering on the balances used in calibration of the instrument, and one, heavy pressure, of 300 gms. Whereas in the former series the subject could not detect the shifting

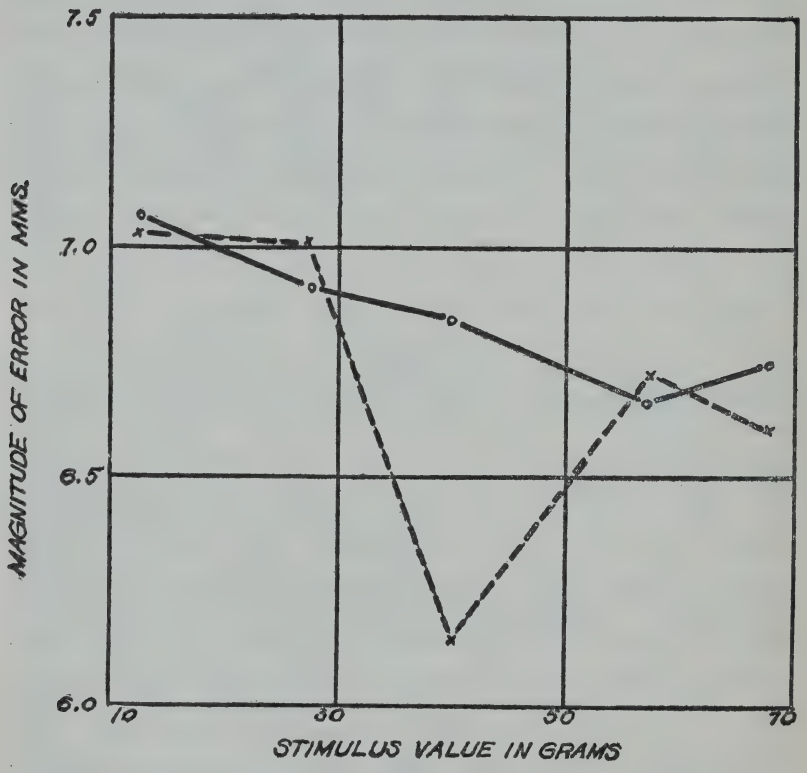

FIGURE 2

GRAPH SHOWING THE RELATION BETWEEN VARIATION IN PRESSURE AND MAGNITUDE OF LOCALIZING ERROR

———— performance for the back of the hand
— — — — performance for the left forearm
Each ordinate represents the mean of 300 localizations by five subjects.

from one pressure to the next (with the exception of the shift from 67.5 gms. to 12 gms.), the transfer was evident here. Two subjects were used, Dr. Dockeray and Dr. Valentine. The same surfaces employed in the preceding series were again investigated. Three spots on each surface were used instead of six, and they were stimulated in irregular order so that the subject would not anticipate the stimulus to be given, either from the pattern of the spots on the surface or from the number of the stimulus. Thirty localizations were again made each day, and the experiment continued until 300 localizations had been made by each subject.

Results. The data appear in Table 4. The results for the single subject for the back of the hand comparing light with heavy pressure show a slight but not significant difference, the heavier pressure being slightly more accurately localized. The same comparison of data for the volar forearm gives similar results. This series of experiments supports the results for the back of the hand given above. *The comparison of light-pressure localizations with heavy-pressure localizations does not give a significant difference.*

Discussion

These results, particularly those for the volar surface of the forearm, are contradictory to those obtained by Franz. This may be due either to differences in technique or to sampling error. Franz's method of localization and measurement of error was subject to much greater error than the present method, even though this one leaves much to be desired. Again, he used different surfaces of investigation, the back, the abdomen, and upper leg, whereas we used the hand and the forearm. That this may account for some of the difference is supported by the fact that in the present investigation there is some discrepancy in the results for the back of the hand and

TABLE 4

Showing the Magnitude of Error, Standard Deviation, and Critical Ratios When the Stimuli "T" and 300 Gms. Were Used

Sub.	Surface of investigation	Magnitude of stimulus	Magnitude of error in mms.	S.D.	C.R. "T"–300 gms.
1	Back of left hand	"T"	5.50	3.2	
		300 gms.	4.64	2.9	1.72
6	Volar surface left forearm	"T"	7.01	4.0	
		300 gms.	5.85	3.6	1.77

the volar surface of the forearm, nor does Franz have consistent results from his various areas. Franz's results were based on 140 localizations, 2 series of 30 localizations each (one subject) and 4 series of 20 localizations each (one subject). In our experiment we have 50 series of 60 localizations each and 4 series of 150 localizations each—eight subjects in all. Even here there are sampling errors as indicated by the performance of Subject 7.

If the localization were dependent upon specific nerve supply as in the usual uncritical interpretation of Lotze's "local sign" doctrine, we would expect the light pressures to be more accurately localized than the heavy pressures since the heavy pressures must obviously stimulate a much larger area and more numerous nerve endings by irradiation and skin deformation. If, however, his "local sign" be more carefully considered as the characteristic intensity pattern, due to the configuration of skin cells, muscle fibers, supporting tissue, blood vessels, bone, and network of nerve fibers lying under the spot stimulated, then the heavier pressures would probably be the more accurately localized since the steeper the pressure gradient the greater would be the shearing strain on the cells adjacent to the center of stimulation, and a consequent more definite stimulation of the cells more directly beneath. The effect of the shearing strain upon accuracy of localization could be attacked experimentally. Our experimental results indicate, as is demonstrated clearly in Figure 2, that light pressures are more inaccurately localized than the heavy ones if *any* differentiation is made. The "local sign," therefore, seems to be just as definite when the stimulating pattern is more complex, widespread, and deeper as when it is extremely punciform and superficial. This, however, is complicated by the localizing movement itself. It was noted by both the subjects and the experimenter that the pressure exerted on the localizing stylus was of the same order as that used by the experimenter. That is, if the subject were stimulated with light pressure he would respond in kind, and if he were stimulated with a heavy pressure his localizing response would involve a heavy pressure. This variable was not controlled, and at present no method is evident for such control. As far as our data are significant at all they show that the localization involving the "deep pressure sense" is at least as accurate as that involving only the skin.

The time element also affects our results. While the time that the stylus was actually in contact with the skin varied very little from one application to the next, the *effect* of the stimulation could

not be considered as constant in duration. For the very heavy pressures the deformation of the skin could be observed by the experimenter to last as long as 20 or 30 secs., and it is probable that more minute but unobservable effects persisted for even longer times. Since this is true, the question of the adjustment of the organism for localization would be quite different for light touch than for heavy pressure. That is, in the former case the localizing movement would be made after the sensory processes resulting directly from the stimulation had been terminated.

The factor of learning is also to be considered. While no definite statement can be made concerning the effects without a study of the development of pressure localization in children of different ages, our results suggest the influence of training. It is generally conceded that we have more experience with localizing stimuli on our hands than on other parts of the body. It may be that learning has overcome any difference that may have existed in the beginning between the localizations of stimuli of different pressures (as on the hand) and that the difference still exists on other parts (as on the volar surface of the forearm). Our present conception of intensity as a learned response just as space is a learned response would not lead one to expect any original difference, however. Such a probability can be resolved by experimentation.

Judgment of Accuracy

Subjects 1, 2, 4, and 6 were required to make judgments as to the accuracy of each localization. In each case the judgments fell into four response categories which have been called in this study ++, very good; +, good; 0, only fair; and —, very poor. When the subjects were asked to give an opinion concerning their general impression as to which was the most accurately localized, light or heavy pressures, they indicated that they believed the heavy pressures to be more accurately localized. That this is actually so is shown by the percentage of "very good" and "good" judgments accompanying the heavier pressures even when they fail to discriminate that a heavier pressure has been used. The results are given in Table 5 and Figure 3. The graph shows that the "very good" and "good" judgments occur a greater percentage of the time for heavy pressures than for light, and that "fair" and "poor" judgments occur more frequently for the light pressures.

TABLE 5

PERCENTAGE OF ACCURACY JUDGMENTS MADE AT EACH STIMULUS LEVEL
(Average for four subjects)

Judgment	12.0 gms.	27.0 gms.	40.0 gms.	57.5 gms.	67.5 gms.
++	5.7	2.8	4.8	9.9	6.3
+	42.6	49.5	59.2	56.0	54.5
0	40.5	42.4	32.6	31.1	35.9
—	10.8	5.1	3.2	2.8	3.2

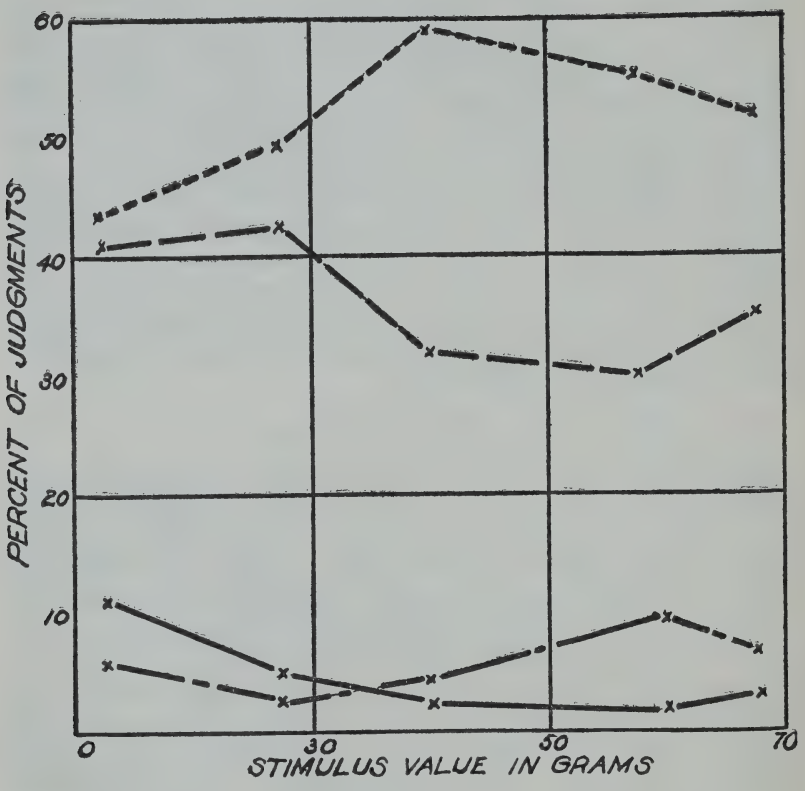

FIGURE 3

GRAPH SHOWING THE JUDGMENT OF ACCURACY OF LOCALIZATION OF STIMULI AT EACH STIMULUS LEVEL

- - - - - - - - + judgments
— — — — — 0 judgments
———————— — judgments
— - — - — - ++ judgments

The correlation between the judgment of accuracy and actual accuracy was also calculated. An r or $+.05$ was obtained, showing absolutely independent variation. An inaccurate localization was just as apt to be judged "good" as "poor."

Summary

An area 10 mm. square was investigated for accuracy of localization on each of the backs of five left hands and on each of five left forearms. Five different pressures were used, 12 gms., 27 gms., 40 gms., 57.5 gms., and 67.5 gms., and 60 localizations of each pressure were made in each case. The results do not support Franz's contention that light pressures are more accurately localized than heavier pressures. For the back of the hand they support Ponzo in his conclusion that the pressure variable is not influential in determining the magnitude of error. For the volar surface of the forearm they are unique, are statistically in favor of medium pressures rather than light pressures for accuracy of localization, emphasize individual differences, and suggest several problems. In addition, the judgment of the individual as to the accuracy of the localization is more indicative of the magnitude of the pressure of the stimulus than of the actual accuracy of localization.

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L'EFFET DE LA GRANDEUR DE LA PRESSION SUR LA LOCALISATION CUTANÉE

(Résumé)

Selon Ponzo, la variation de la pression de la stimulation tactile n'a aucune influence sur la précision du mouvement localisant. Franz a constaté que les pressions légères ont été plus précisément localisées que les pressions lourdes. On a fait cette étude dans le but de fournir d'autres données sur l'effet de la grandeur de la pression sur l'erreur de la localisation. On a obtenu la variation de pression au moyen d'un stylet spécialement construit qui a opéré selon le principe d'un ressort compresseur. Le point de stimulation du stylet a eu un diamètre de 0,07 mm. On a employé huit sujets dans l'expérience. On a étudié des aires de la partie dorsale

de la main gauche et de la surface inférieure de l'avant-bras gauche. On a fait des localisations selon la méthode tactile-kinesthésique de Weber. On a employé sept pressions différentes: 'T', assez légère pour ne pas s'enregistrer sur la balance employée pour le calibrage de l'instrument, 12 gms., 27 gms., 40 gms., 57,5 gms., 67,5 gms., et 300 gms. On a fait 3600 localisations. Les résultats, comme ensemble soutiennent les résultats de Ponzo. Ils contredisent spécifiquement Franz puisque les pressions légères n'ont jamais été plus précisément localisées que les pressions lourdes. Enfin, on a demandé à quatre sujets de juger la précision de la localisation. Les résultats montrent que les pressions lourdes ont tendu à faire venir la résponse 'bonne' et les pressions légères 'assez bonne', tandis que la précision du mouvement losalisant lui-même n'a pas influencé le jugement, paraît-il.

DISHER

DIE EINWIRKUNG DER STÄRKE DES DRUCK AUF DIE KUTANE LOKALISIERUNGSFÄHIGKEIT

(Referat)

Nach Ponzo hat Änderung im Druck bei der Tastreizung [tactual stimulation] keine Einwirkung auf die Genauigkeit der lokaliserenden Bewegung. Franz fand, dass die sanfteren Drucke genauer lokalisiert wurden, als die schweren. Die Absicht dieser Untersuchung war, weitere Befunde zu sammeln in Bezug auf die Einwirkung der Druckstärke auf Lokalisierungsfehler. Die Druckänderung wurde mit einem besonders konstruirten Griffel bewirkt, der nach dem Prinzip der Springfederkompression [spring compression principle] funktionierte. Der Reizpunkt der Griffels hatte einen Durchmesser von .07 mm. Es dienten 8 Versuchspersonen. Es wurden Flächen auf dem linken Handrücken und auf der Vorderfläche des linken Vorderarms untersucht. Die Lokalisierungen wurden mit der Weber'schen taktil-kinesthetischen Methode ausgeführt. Es wurden sieben verschiedene Druckstärken verwendet: 'T,' so sanft, dass er sich auf der zur Kalibrierung des Instrumentes verwendeten Wage nicht registrierte: 12 Gr.; 40 Gr.; 57.5 Gr.; 67.5 Gr.; und 300 Gr. Es wurden 3600 Lokalisierungen gemacht. Die Befunde bestätigen im Allgemeinen Ponzo's. Sie widersprechen spezifisch die Befunde von Franz, da die sanften Drucke nie genauer lokalisiert wurden, als die schweren. Schliesslich hiess man 4 Versuchspersonen Urteile fällen in Bezug auf die Genauigkeit der Lokalisierung. Die Befunde beweisen, dass die schweren Drucke im Allgemeinen die Reaktion "Gut," und die leichten Drücke im Allgemeinen die Reaktion [response] "Ziemlich gut" hervorriefen, wahrend die Genauigkeit der lokalisierenden Bewegung selber das Urteil scheinbar nicht beeinflusste.

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PERIPHERAL FORM DISCRIMINATION UNDER DARK-ADAPTATION*[1]

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I. INTRODUCTION

The present study of the nature and extent of the peripheral form field is a continuation of a preliminary experiment reported by Munn and Geil (8) in 1931. Certain defects in the original apparatus and technique made a continuation of the investigation advisable. The apparatus used by Munn and Geil possessed the following defects: (1) the exposure time of the stimulus was inadequately determined, (2) the background for the stimuli was not controlled, (3) the illumination of the experimental room was from an overhead source, causing the stimuli to be shaded at certain positions in the field of vision, and (4) the method of presenting the stimuli allowed the subject an opportunity to remember the form whose identity was already established rather than to discriminate the forms as they were presented.

There have been comparatively few published reports of investigations of peripheral form discrimination, hence an historical summary of the work would of necessity be brief. Such summaries have been made by Kleitman and Blier (7), and by Collier (2). There are some significant differences between our methods and those used by other experimenters. All the published investigations, except the one by Munn and Geil (8), involved discrimination of the stimuli with the use of one eye. Our subjects used both eyes and discriminated two stimuli presented simultaneously, each at a corresponding location on a given meridian in the field of vision. Other

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investigations have been made under some general illumination. Our study was made under conditions of dark-adaptation. Kleitman and Blier (7) and Zigler, Cook, Miller, and Wemple (11) used a moving stimulus.

In all of the published studies of peripheral form discrimination the exposure time of the stimulus has been inadequately controlled. The advantages of a momentary exposure of the stimulus have been discussed by Ferree and Rand (4) and the fallacy in the results of discriminating a continuously exposed stimulus is made evident in a phenomenon described by Dunlap (3). Johnson (6, p. 207) has discussed this phenomenon as "evidence consistent with the theory of excitation by 'drainage' "—and, in a discussion of its bearing on perimetry, gave the writer the following written comment. "The phenomenon suggests that in practical life we make judgments upon stimuli which are exposed *virtually* for brief periods only, as Cobb has pointed out; but still worse, because of the high speed with which the blinding effect occurs in the peripheral field, a particular determination of acuity—or for that matter, a particular judgment of visibility or non-visibility—means nothing unless the virtual exposure time is taken account of. Since eye movements may interrupt exposure at any instant, and since in such experiments as these, the fixational pause cannot be determined, it would seem that brief exposures are always demanded. Even so, could the state of adaptation of the stimulated portion be ascertained unless we know how long the eye had been fixed at the instant that the stimulus was exposed? A nice problem in itself!"

Experimenters have disagreed in statements concerning the general size of the form field and the relative sizes of the fields for particular forms. These different results might well be influenced by the difference in the size and brightness of the stimulus objects used and the physical conditions under which the stimuli were presented. In none of the published reports of investigations of peripheral form discrimination has the writer found any statement of the brightness of the stimulus in terms of standard units.

II. Apparatus and Methods of Investigation

The apparatus used in this investigation was essentially that described by Munn and Geil (8). The primary changes made in the apparatus they describe were the addition of a background for

the stimuli and a stimulus carrier which facilitated the changing of the forms after each exposure.

The stimuli were translucent surfaces in the form of a circle, a triangle, a diamond, a hexagon, a rectangle, and a square, respectively, each having an area of 10 square centimeters. The brightness of each stimulus, determined by the Macbeth illuminometer, was 244 millilamberts. The stimuli were prepared by cutting the forms from heavy cardboard and covering the face of the aperture with white paper. The lamp housing was directly behind the covered aperture so that when the lamp was illuminated the stimuli became visible. Ten-watt daylight Mazda bulbs connected to the 110-volt direct-current power plant in the laboratory provided the illumination for the stimuli.

A head clamp was arranged on the subject's chair so that adjustable padded jaws fitted just in front of the subject's ears. The forehead and chin pieces of the clamp were adjusted so that the perpendicular position of the subject's head might be maintained with a minimum of effort. The distance from the midpoint between the subject's eyes to the stimuli was 60 centimeters.

The duration of the contact time of the circuit through the stimulus lamps was controlled by an apparatus described by Whitmer (10). A rheostat was arranged so that a weak current was allowed to pass continuously through the lamps to keep the filament heated. The actual duration of the contact time of the circuit leading to the stimuli was measured by a Dunlap chronoscope and for 100 contacts the average time of .376 seconds ranged from .366 to .390 seconds with a standard deviation of .005 seconds. This stated time was chosen with an aim to make the time of exposure of the stimuli short enough so that if the subject changed his fixation it would materially reduce the probability of his being able to discriminate both forms. Other factors considered in limiting the exposure time of the stimuli were (1) the threshold time of exposure, (2) the action time of the lamps after the current had been turned on, and (3) the rate of shift in eye-movements. The first factor was considered in the light of an investigation by Cobb and Moss (1) who say that "the threshold of time exposure of a test object has been found, in general, to be shorter with higher illumination," but that "beyond, say, 0.3 seconds the time of exposure is hardly critical." In the present study the time was held virtually constant and, since it was not to be considered a critical factor in the discrimina-

tion, so long as fixation was maintained, we limited the exposure time to approximately 0.3 seconds. In order to reduce the action time of the lamps their filaments were kept heated to a glow just below the threshold of visibility to the dark-adapted eye.

Illumination for the recording of responses was furnished by a flashlight covered wtih red crepe paper. The fixation-point was a pin-hole in an opaque box which contained a lamp of 3 candle power. The hole was covered by a piece of red paper to reduce the brightness.

The subjects were dark-adapted for 45 minutes before each period of observation. The three subjects used in the experiment had normal vision according to the Snellen test cards, and none of them had ever worn glasses.

The stimuli were presented in chance pairs, i. e., sometimes like and sometimes different forms each at a corresponding location in each of the quadrants of a single meridian. The stimuli were changed after every exposure so that the next exposure demanded a new discrimination. The procedure for the investigation of a meridian was as follows: Each pair of forms was presented (1) 3 times at each 10-degree interval beginning at 20 degrees and proceeding outward toward the periphery, (2) 3 times at each 10-degree interval beginning at the outer 10-degree interval and proceeding inward, (3) 2 times progressing as in (1), and (4) 2 times progressing as in (2). This made a total of 10 presentations of each form at the designated position in the field of vision. The outer limit of the field of vision was taken as the 10-degree interval at which the subject reported no discrimination except spots of light.

The subjects were given a signal of "ready" before the exposure of the stimuli and were cautioned not to maintain fixation except at the signal. The subjects responded by naming the forms which were presented and were not informed of the correctness of their responses.

The periods of observation were limited to approximately one hour, not including the period for dark-adaptation. Frequent rest periods were permitted as the apparatus was changed for testing at the various locations in the visual field.

III. Results

Results presented in the study represent the discriminations made in a total of 10,140 exposures of the stimuli. Discriminations were

made by three subjects in eight quadrants of the visual field. The responses of all the subjects are combined in the tables of results. The extreme interval of the form field was found to differ among the subjects and the meridians. The discriminations made at the extreme interval on any quadrant are presented in the combined results regardless of individual variations, that is, the given percentage in the results was computed on the basis of 30 responses and, if the given position was outside the form field for any single subject, his portion of the responses are counted incorrect. This individual difference affects the tables of results in the last two intervals of the horizontal quadrants and the single extreme interval on all the other quadrants.

Figure 1 is a representation of the scheme for indicating positions

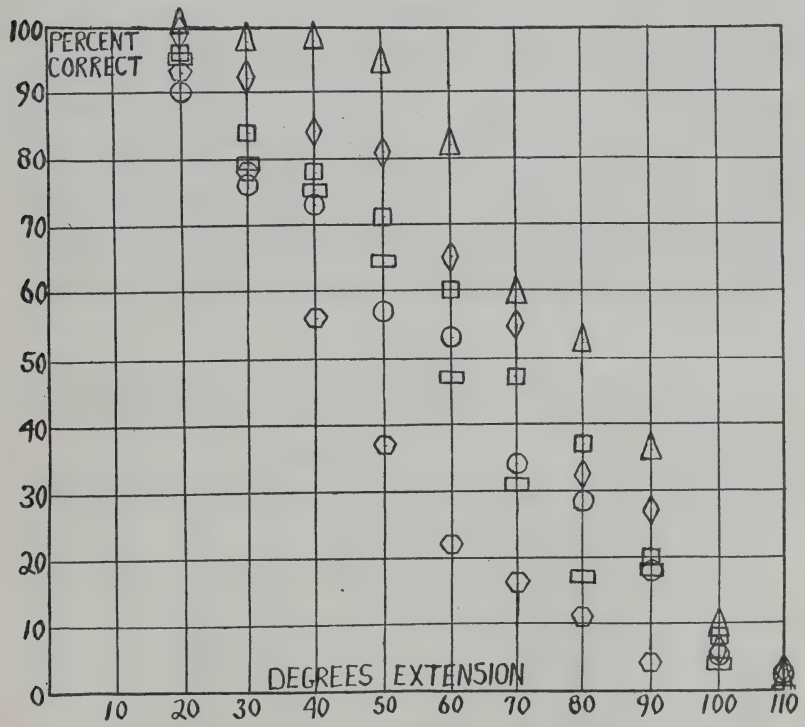

FIGURE 1

SCHEMATIC REPRESENTATION OF POSITIONS IN THE VISUAL FIELD

of the stimuli in the field of vision. All positions are represented as locations in the visual field and not as positions on the retina.

The names assigned to the positions indicated in Figure 1 are as follows: I is the right quadrant of the horizontal meridian; II, the upper right diagonal quadrant; III, the upper vertical quadrant; IV, the upper left diagonal quadrant; V, the left horizontal quadrant; VI, the lower left diagonal quadrant; VII, the lower vertical quadrant; and VIII, the lower right diagonal quadrant. The Roman numerals indicating the quadrants are used for the sake of convenience in the tables of results. The location of positions of extension on any quadrant are calculated on arcs whose common zero point is the point of fixation in the visual field. The midpoint between the subject's eyes is the center of the circle of which any of the meridianal arcs are a part.

Tables 1-6 show the percentage of correct discriminations for each of the forms at the designated locations in the field of vision.

The general tendency in all these tables is toward fewer correct discriminations as the degree of extension is increased toward the periphery of the field of vision. A slight exception to this tendency appears in some of the tables where the accuracy is greater at 90 degrees than at 80 degrees. This exception is consistent in the table for the diamond. In general the exception is more pronounced, particularly in the individual results, when the stimuli were presented on a meridian where one stimulus was outside the field of

TABLE 1

THE PERCENTAGE OF CORRECT DISCRIMINATIONS OF THE TRIANGLE AT 10-DEGREE INTERVALS, BEGINNING AT 20-DEGREES EXTENSION ON EACH QUADRANT

Degrees extension	Quadrants I	II	III	IV	V	VI	VII	VIII
20	100*	100	100	100	100	100	100	100
30	100	93	97	100	100	97	97	97
40	97	90	100	100	100	100	100	100
50	97	97	73	97	97	97	97	100
60	97	87	10	87	97	97	90	97
70	93	57	..	53	100	80	70	77
80	93	33	..	13	93	80	27	73
90	73	..	..	..	73	70	..	67
100	17	..	..	..	10	30	..	20
110	10	..	..	..	7	..	..	..

*In Tables 1-7 percentages above 17 are greater than chance.

TABLE 2

THE PERCENTAGE OF CORRECT DISCRIMINATIONS OF THE DIAMOND AT 10-DEGREE INTERVALS, BEGINNING AT 20-DEGREES EXTENSION ON EACH QUADRANT

Degrees extension	Quadrants I	II	III	IV	V	VI	VII	VIII
20	100	100	97	97	100	100	100	100
30	100	100	87	90	80	100	93	83
40	87	83	70	87	80	100	83	83
50	97	90	50	83	80	87	83	80
60	90	53	23	50	77	80	67	80
70	77	43	..	57	67	93	47	60
80	57	20	..	7	47	53	30	47
90	60	..	..	..	53	57	..	50
100	10	..	..	..	3	23	..	10
110	13	..	..	..	10	..	..	..

TABLE 3

THE PERCENTAGE OF CORRECT DISCRIMINATIONS OF THE SQUARE AT 10-DEGREE INTERVALS, BEGINNING AT 20-DEGREES EXTENSION ON EACH QUADRANT

Degrees extension	Quadrants I	II	III	IV	V	VI	VII	VIII
20	100	100	83	100	100	100	93	97
30	97	93	53	90	93	93	60	97
40	93	80	43	80	90	97	60	87
50	80	80	27	70	83	90	70	70
60	67	60	17	63	90	73	47	63
70	53	30	..	43	87	60	30	53
80	50	10	..	17	57	47	33	33
90	27	..	..	..	37	60	..	40
100	23	..	..	..	7	17	..	20
110	7	..	..	..	3	..	..	..

TABLE 4

THE PERCENTAGE OF CORRECT DISCRIMINATIONS OF THE RECTANGLE AT 10-DEGREE INTERVALS, BEGINNING AT 20-DEGREES EXTENSION ON EACH QUADRANT

Degrees extension	Quadrants I	II	III	IV	V	VI	VII	VIII
20	100	100	87	90	100	100	90	97
30	90	83	63	63	90	73	83	87
40	73	60	63	73	93	77	83	77
50	67	57	60	43	70	63	73	83
60	43	30	20	37	57	73	47	67
70	50	30	..	20	40	43	13	50
80	20	20	..	0	37	40	10	27
90	60	..	..	..	50	20	..	17
100	20	..	..	..	3	0	..	10
110	0	..	..	..	3	..	..	..

TABLE 5

THE PERCENTAGE OF CORRECT DISCRIMINATIONS OF THE CIRCLE AT 10-DEGREE INTERVALS, BEGINNING AT 20-DEGREES EXTENSION ON EACH QUADRANT

Degrees extension	Quadrants I	II	III	IV	V	VI	VII	VIII
20	100	93	87	90	100	93	80	80
30	83	73	70	77	87	90	63	63
40	80	90	73	70	67	80	57	67
50	63	73	37	73	70	67	33	40
60	57	50	17	73	80	60	40	47
70	40	20	..	27	53	57	20	60
80	43	17	..	17	50	40	27	37
90	33	..	..	..	40	33	..	40
100	10	..	..	..	7	13	..	7
110	7	..	..	..	7	..	..	..

TABLE 6

THE PERCENTAGE OF CORRECT DISCRIMINATIONS OF THE HEXAGON AT 10-DEGREE INTERVALS, BEGINNING AT 20-DEGREES EXTENSION ON EACH QUADRANT

Degrees extension	Quadrants I	II	III	IV	V	VI	VII	VIII
20	100	90	90	87	100	93	90	97
30	80	70	60	93	90	97	60	77
40	77	40	30	57	87	77	33	50
50	57	27	3	40	60	67	23	20
60	23	7	0	10	40	60	17	20
70	17	0	..	7	53	37	7	7
80	20	3	..	0	33	27	0	7
90	7	..	..	..	13	3	..	10
100	0	..	..	..	0	0	..	0
110	0	..	..	..	0	..	..	..

vision. This situation happened when the stimuli were presented on the diagonal and vertical meridians. There are several possible reasons for the increase in correct responses when but one of the stimuli was in the form field. The discrimination of two forms at widely separated positions in the field of vision is a more complex task than the discrimination of only one stimulus and thus more errors may be inevitable. The factor of "attention" to one stimulus instead of two may allow a greater percentage of correct discriminations. A further possibility might be in the loss of fixation when the stimulus appeared in only one side of the field. The subjects were cautioned not to shift their eyes during the period immediately after the "ready" signal had been given. One fact which allows

us to think that the loss of fixation was not a factor is that the higher percentage of correct responses at certain intervals than at more central points did not appear in the results for all forms.

Comparison of the preceding tables shows that the hexagon ranked lowest of any of the forms in percentage of correct discriminations. Very few correct responses were made to this form in the outer 30 degrees of the peripheral field on any quadrant. The general tendency toward decrease in accuracy of discrimination for the rectangle, square, and circle may be said to be gradual as the forms were displaced farther toward the periphery. Discrimination of the hexagon drops sharply at a more central position, then follows with a gradual decline in accuracy. The decline in accuracy for the triangle, and

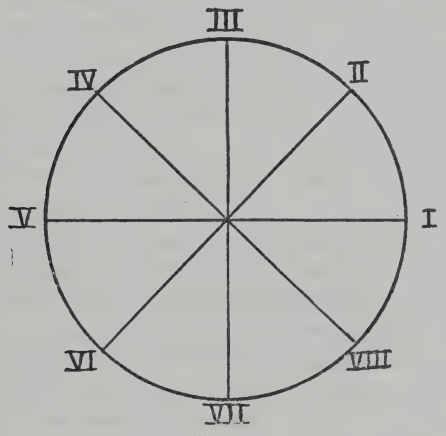

FIGURE 2

SHOWING THE RELATIVE DISCRIMINATION VALUE OF EACH FORM AT THE VARIOUS INTERVALS OF EXTENSION ON ALL QUADRANTS COMBINED*

*In combining the data for all meridians at any single degree of extension greater than 60 degrees some of the positions on particular meridians were outside the field of vision. The forms are located at the given extensions in Figure 2 on the basis of a percentage computed by counting 30 failures to discriminate at each position outside the field at that interval. Other investigators, Kleitman and Blier (7) and Munn and Geil (8), for example, computed the average extension of the forms they used by averaging the extreme degree of extension on each quadrant where the form was discriminated. The average extension computed thus does not represent the actual amount of discrimination on any single quadrant. Our presentation of the data in this form is made to show only the relative discrimination values of the different forms at the various extensions in the entire visual field. The actual percentage for each form is not given because, in the writer's estimation, it has no definite meaning.

to a somewhat lesser extent for the diamond, can be identified as a very gradual loss for several intervals followed with a pronounced loss in percentage of accuracy.

Figure 2 is a graphic representation of the accuracy value of each form in the entire visual field.

The significant fact in the foregoing figure is that some of the forms hold a fairly constant relative rank in discrimination value. For example, the triangle has the highest and the hexagon the lowest rank from 30 degrees out to the 90-degree extension in the peripheral field. The relative ranks for the other forms are not constant but they maintain an intermediate position in discrimination value.

The relative rank of the six forms in the whole field investigated, in terms of percentage of accuracy, is as follows: triangle 86, diamond 73, square 66, rectangle 57, circle 57, and hexagon 40. These percentages were obtained from the percentage of correct discriminations made by the three subjects. These differences, with the exception of the rectangle and the circle, are statistically true differences. In this fact our results do not agree with those of Kleitman and Blier (7), who found that the perimeters of the circle, triangle, square, and five-point star used in their experiment were practically identical. The difference may be due to the facts that they worked in daylight, their forms were inscribed in circles of equal diameter, and that they used a moving stimulus with the consequent possibility of loss in extreme peripheral vision under steady fixation. The visibility rank of the forms used in our study was approximately the same as the rank in average extension found by Munn and Geil (8). Our results also agree with those of Munn and Geil in the fact that the triangle was much more easily discriminated and the hexagon much more difficult to discriminate than were the other forms. In the investigation by Collier (2) the order of the percentage-correct values of the forms was approximately the same as in our experiment in spite of the fact that his forms were equal in the distance between their most widely separated points, rather than in area. The actual percentage of correct discriminations of the various forms used in Collier's experiment and our own was approximately the same.

Both Collier's and our results support the statement by Munn and Geil (8, p. 85) that the retinal distribution of light emitted by a form seemed to be the important factor in determining the distance from the fovea at which it could be discriminated. This interpreta-

tion seems to be satisfactory for conditions of dark-adaptation as well as when there is general illumination in the experimental room. Momentary illumination of the stimuli may also have been a common factor in determining the similarity of results in these three experiments.

In an experiment by Geissler (5) the forms were exposed for two or three seconds. This length of exposure probably invalidated any claim that can be made for the efficacy of momentary illumination as a method of presenting the stimuli. Giessler's results, stated very briefly, are at variance with ours. He used a circle, a square, a triangle, and a sector, each approximately 80 square millimeters in area. In his results the circle was most often and the sector least often discriminated while our data show the circle to have a low accuracy value as compared with the triangle. This again raises the question as to whether or not a method which allows long exposure of the stimuli is adequate for the study of indirect vision. We do not believe that any method which employs a long exposure of the stimuli is satisfactory for studying indirect vision since, even though the fixation may be controlled, the peripheral field may blot during a long period of steady fixation.

In all of the investigations of peripheral form discrimination discussed above, except that by Munn and Geil (8) and our own, the form was presented to one eye of the subject. The presentation of the stimuli in pairs might seem to increase the accuracy of discrimination when both forms were in the portion of the visual field for both eyes. In our experiment the forms were in the field of vision for both eyes out to approximately 60-degrees extension. Our data show no consistent change at the intervals where the stimuli were restricted to a single eye for each stimulus.

According to our results no form used, except the hexagon, could be said to have a restricted area within the field of vision for form. All forms, except the hexagon, were discriminated within a fairly well-defined field but with more or less accuracy depending upon the definiteness of the outline of the form.

Table 7 shows the combined accuracy of discrimination of all forms used. The results are presented in this manner in order to show the relative efficiency of discrimination on the various quadrants of the field of vision.

Inspection of Table 7 shows the decrease in accuracy of discrimination to be most rapid on the quadrants which have the least exten-

TABLE 7

THE PERCENTAGE OF CORRECT DISCRIMINATIONS OF ALL FORMS AT 10-DEGREE INTERVALS, BEGINNING AT 20-DEGREES EXTENSION ON EACH QUADRANT

Degrees extension	Quadrants I	II	III	IV	V	VI	VII	VIII
20	100	97	91	94	100	98	92	95
30	92	85	72	86	90	92	76	84
40	84	74	65	78	86	88	69	77
50	77	71	42	68	77	78	63	65
60	63	48	14	53	73	74	51	62
70	55	30	..	35	67	61	31	51
80	47	17	..	9	53	48	21	38
90	43	..	..	..	45	40	..	37
100	13	..	..	..	5	14	..	11
110	6	..	..	..	5	..	..	..

sion in the field of vision. Conversely, the decrease in accuracy is least rapid on the quadrants which have the greatest extension. The superiority of vision in the horizontal and lower diagonal quadrants of the peripheral field is evident. The extent of the vertical and upper diagonal quadrants is limited by facial conformations, but even when the stimuli are well within the field of vision the accuracy of discrimination is less on these quadrants. The decrease in accuracy on the various quadrants may be summarized as follows: least rapid on the horizontal and lower diagonal quadrants, more rapid on the upper diagonal and lower vertical quadrants, and most rapid on the upper quadrant of the vertical meridian. Wertheim (9) pointed out these tendencies in a study published in 1894, and other investigators have found similar results. The fact that the same tendencies appear when the discriminations are made with both eyes simultaneously, as the stimuli are presented on opposite sides of the meridian, is doubly significant of the relative efficiency of discrimination at the various positions in the visual field.

When the subjects in our investigation responded to the exposure of the stimuli their responses were recorded along with the name of the form exposed. These responses are classified in Table 8, in order to show the amount and type of confusion among the different forms.

Table 8 should be read as follows: of the total number of exposures of the triangle 86% were named correctly, 6% were called "diamond," 3% were called "square," 2% "circle," etc.

The most outstanding fact was that the hexagon, least often cor-

TABLE 8
PERCENTAGE OF TIMES THE FORMS WERE NAMED IN THE TOTAL NUMBER OF EXPOSURES OF EACH FORM

Forms exposed	Tri-angle	Dia-mond	Square	Cir-cle	Rec-tangle	Hex-agon	No form
Triangle	**86**	6	3	2	1	1	1
Diamond	9	**73**	6	6	3	2	1
Square	7	7	**66**	6	10	2	2
Circle	6	11	8	**57**	7	9	2
Rectangle	6	6	23	5	**57**	1	2
Hexagon	8	13	9	22	6	**40**	2

rectly discriminated, was also least often named as a response to other forms. In general, the forms which were neither lowest nor highest in accuracy value were named in the incorrect responses. The tendency toward confusion of the forms which resemble each other in outline accounts for the greater part of the errors. The exception to this statement is that the circle, which seems similar to the hexagon, was not called "hexagon" when it was incorrectly named. The reverse of this was true, however, as the hexagon was most often called "circle" when it was incorrectly named.

IV. CONCLUSIONS

1. The rank of the forms in percentage of correct discrimination was: triangle, diamond, square, rectangle-circle, and hexagon.

2. The field of peripheral vision for all the forms except the hexagon was approximately the same in extent but varied in accuracy for the different forms.

3. The extent of the peripheral field for the hexagon was from 10 to 20 degrees less than for the other forms.

4. The decrease in accuracy of discrimination was most rapid on the upper vertical quadrant, less rapid on the lower vertical quadrant and the upper diagonal quadrants, still less rapid on the lower diagonal quadrants, and least rapid on the horizontal quadrants.

5. The decrease in accuracy of discrimination was least rapid on the quadrants which had the greatest extension in the field of vision and most rapid on the quadrants which had the least extension.

6. No consistent variation in accuracy of discrimination occurred at the intervals of extension where the stimuli passed from the field of vision for both eyes to a separate field for each eye.

7. Errors in naming the forms tended to be associated with the

form most nearly like the form exposed, except that the circle was not called "hexagon" as many times as it was called "diamond."

8. The hexagon was discriminated correctly less times than any of the other forms and was also named the least number of times in the attempts to discriminate other forms.

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LA DISCRIMINATION PÉRIPHÉRALE DES FORMES SOUS L'ADAPTATION OBSCURE

(Résumé)

Six formes (triangle, diamant, carré, rectangle, cercle, et hexagone), chacune d'une aire de 10 centimètres carrée, ont été exposées en combinaisons en paires, une à chaque oeil, à une distance de 60 centimétres. Les formes ont été exposées au moyen d'une lumiére apparaissant subitement derrière une surface translucide, dont la clarté a été de 244 millilamberts. La durée de l'exposition de chaque paire de formes a été de 0,3 secondes. Trois sujets à la vision normale, adaptés à l'obscurité pendant 45 minutes avant le commencement de chaque période d'observation, ont fait 10,140 réponses aux formes. L'ordre de la précision de la discrimination des

six formes a été celui donné ci-dessus. Les résultats ont montré un décroissement plus ou moins graduel de la précision avec l'extension vers la périphérie, dépendant de la forme employée. L'etendue périphérale du champ pour toutes les formes sauf l'hexagone a été approximativement la même. Le champ pour l'hexagone a été de 10 à 20 degrés de moins que pour les autres formes dépendant de quadrants spéciaux. La discrimination a été la plus précise et l'étendue du champ la plus grande sur les quadrants horizontaux et les quadrants inférieurs diagonaux. La précision de la discrimination n'a pas été variée d'une manière constante dans cette portion du champ où les stimuli ont passé du champ de vision pour les deux yeux à un champ séparé pour chaque oeil. Les erreurs qu'on a faites en nommant les formes ont tendu à être associées à la forme ressemblant le plus à celle exposée.

WHITMER

DIE UNTERSCHEIDUNG PERIPHERISCHER FORMEN UNTER DUNKELADAPTION

(Referat)

Sechs Formen (Dreieck, Rhombus, Viereck, Rechteck, Kreis und Sechseck), von denen jede eine Fläche von zehn Quadratzentimetern hatte, wurden paarweise, jedem Auge eine, in einer Entfernung von 60 Zentimetern dargeboten. Die Formen wurden durch Aufblitzen eines hinter einer durchsichtigen Fläche gestellten Lichtes exponiert, dessen Helligkeit 244 Millilambert war. Die Expositionszeit für jedes Paar der Formen war 0.3 Sekunden. Drei Versuchspersonen mit normalem Sehvermögen, die vor Anfang jeder Beobachtungsperiode 45 Minuten lang auf Dunkelheit eingestellt wurden, reagierten 10140 mal auf die Formen. Die Rangordnung der sechs Formen in der Unterscheidungsgenauigkeit stimmt überein mit der Reihenfolge der obengenannten Formen. Die Ergebnisse zeigten eine mehr oder weniger allmähliche Verminderung in der Genauigkeit je nach der dargebotenen Form mit der Ausdehnung gegen die Peripherie. Der peripherische Umfang des Gebietes für alle Formen, ausser dem Sechseck, war ungefähr gleich. Das Gebiet für das Sechseck war von zehn bis zwanzig Grad geringer als das der anderen Formen, je nach dem besonderen Quadranten. Die Unterscheidung war am genauesten, und der Umfang des Gebietes war der grösste auf den horizontalen und unteren diagonalen Quadranten. Die Genauigkeit der Unterscheidung verändert sich in jenem Teil des Gebietes nicht gleichmässig, wo die Reize vom Gesichtsfeld beider Augen zu einem getrennten Gebiet eines einzelnen Auges übergingen. Die bei der Benennung der Formen gemachten Fehler hatten die Tendenz mit der Formverassoziiert zu werden die der dargebotenen am ähnlichsten war.

WHITMER

"RESISTANCE TO EXTINCTION" IN THE PROCESS OF CONDITIONING*

From the Laboratory of General Physiology of Harvard University

B. F. SKINNER[1]

We have distinguished elsewhere between the *immediately observed strength* of a conditioned reflex and its *resistance to extinction* (3). The former is evaluated from some quantitative aspect of the reflex at a given elicitation, while the latter is inferred from the properties of an extinction curve taken subsequently. There is no simple relation between them. Under repeated reinforcement, for example, a reflex will continue to develop resistance to extinction after its strength has reached an effectual maximum.

A reinforcing stimulus thus has two distinguishable effects. The traditional conditioning experiment, however, is apparently always confined to one of them—the increase in strength—probably because it is the immediately observable change. But the development of resistance to extinction is equally important. Indeed, it could be argued that extinction is the only appropriate measure of conditioning, since it is concerned with the critical defining property of the process, i.e., the presence or absence of a reinforcing stimulus. But we cannot be dealing with interchangeable measures of a single process, so long as the two "measures" behave to any extent independently, as we have seen to be the case. There are two distinct effects, and we must ignore neither of them.

Where it is desired to determine simply whether *any* conditioning has taken place or has persisted after a period of time, either change alone may suffice. Contrary to established practice, the use of the extinction curve is here usually to be preferred. In order to measure the strength of a reflex we must elicit it, and, if it is a conditioned reflex, the elicitation must be either reinforced or unreinforced. In either case there is an effect upon the state of the reflex, altering it from the condition in which we are attempting to meas-

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[1]National Research Council Fellow.

ure it. This is an unavoidable dilemma, the consequences of which will be more or less serious according to the method used and the relative importance of the effect of a single elicitation. Of the special cases that arise we shall discuss a single example.

In a previous study of conditioning (2), the rate of elicitation of a reflex under continuous stimulation was used as a measure of its strength. In certain cases this was found to reach essentially a maximal value after only one elicitation of the conditioning sequence.[1a] But in determining to what extent the change in strength is due wholly to the first elicitation, the only available "rate" is given by the interval between the first two responses, since the second elicitation contributes a further conditioning effect of its own. We need a statistical demonstration that this interval is shorter than would be observed if the first response were not reinforced. A single experiment cannot be convincing. This is unfortunate, since the amount of conditioning actually observed after one elicitation varies greatly between cases (as we shall note later) and in averaging together a group of first intervals we unavoidably obscure the exceptional examples, which are of special interest. Moreover, although a statistical demonstration in the present case does show that conditioning may take place at one elicitation, we are faced with the comparative unreliability of a single interval as a measure of the extent of the change. In such a case the use of the extinction curve is clearly indicated.

The present paper has two purposes: (1) to demonstrate the use of resistance to extinction in revealing the presence of some amount of conditioning; and (2) to confirm our earlier conclusion that for the organism and the type of reflex here considered a significant effect is definitely observable after only one elicitation of the conditioning sequence.

The apparatus and method have been described in detail elsewhere (1, 2) and the arrangements need be only summarized here. A few minor changes will be noted. The essential part of the apparatus is a small horizontal lever projecting from one wall of a sound-proof experimental box, which may be pressed downward by a rat. When this is done a magazine releases a uniform pellet of food into a convenient tray. The magazine may be disconnected

[1a]The results given in that paper have since been confirmed with about twenty rats.

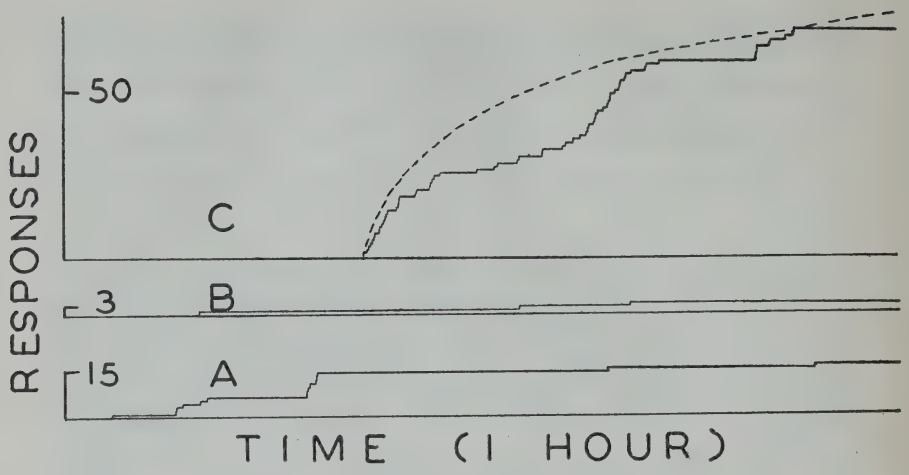

FIGURE 1

by the experimenter. The movements of the lever are recorded on a kymograph drum in the form of a summation curve. The accompanying figures are reproduced from such records, to which coordinates have been added.

The rat[2] becomes fully accustomed to the experimental box during several days on which it eats its daily ration therein. On one or more of these days the lever may be present and investigatory responses to it recorded. On three further days (the lever being absent) pellets of food are periodically delivered from the magazine by the experimenter, and during this time the rat comes to approach the tray immediately after the magazine has sounded and to seize and eat the pellet. The rat is then ready for conditioning. At the usual hour it is placed in the box. For the first time in its history, the movement of the lever will be followed by the delivery of food. *When one response has been made and reinforced, the magazine is disconnected.*

In Figure 1 the control record *A* is of investigatory responses to the lever during a preliminary training period in the absence of food. The responses fall into two groups (probably a facilitative effect) and soon adapt out quite completely. Record *B* is a control

[2]Albino rats of the King inbred strain are used throughout.

record of the investigatory responses in the absence of food after training to the sound of the magazine. Its significance will appear later. Record *C* was taken on the next day. The first investigatory response occurred in about 20 minutes. It was triply reinforced. That is to say, one pellet of food was discharged simultaneously with the response, and two more were discharged by the experimenter within three seconds. Although the rat received three pellets of food, *only one response was reinforced.* It will be seen from the record that the rat began to respond again immediately and during the next 40 minutes traced out a typical extinction curve. We are forced to conclude that this is the effect of the single reinforcement.[3]

The record has the general properties reported for extinction (3). A logarithmic envelope may be drawn above the curve (the broken line is for the equation $N = K \log t$, where N is the number of responses at time t and K is a constant), but the experimental curve suffers a cyclic deviation below it. The present example is an exceptionally good demonstration of this wave-like character, which was previously inferred from records much less regular (3). The curve differs in one respect from those obtained after more extensive conditioning: it lacks the initial burst of responses that lie upon the theoretical curve. To fit Figure 1 in this way it would be necessary to shift the origin of the theoretical curve about one minute to the right. This may indicate a lag in the effect of the conditioning.

The result is not always so clear-cut. In perhaps a majority of cases the control line *B* fails to maintain the slope to be expected from *A*. That is to say, the training to the sound of the magazine produces an increase in the observed rate of responding that is independent of any actual reinforcement with the magazine. A new base-line appears, of which the first part of the record in Figure 2 is an example. Although no reinforcement with the magazine had yet taken place, a significant rate of elicitation is evident. The extinction curve must be observed against this new base-line. At the arrow in Figure 2 a single response to the lever was reinforced with one pellet of food. After a short pause the curve of extinction was begun. Assuming that the base-line is approximately linear, we may subtract it from the experimental curve. The dotted curve

[3]Cf. (4), Appendix II.

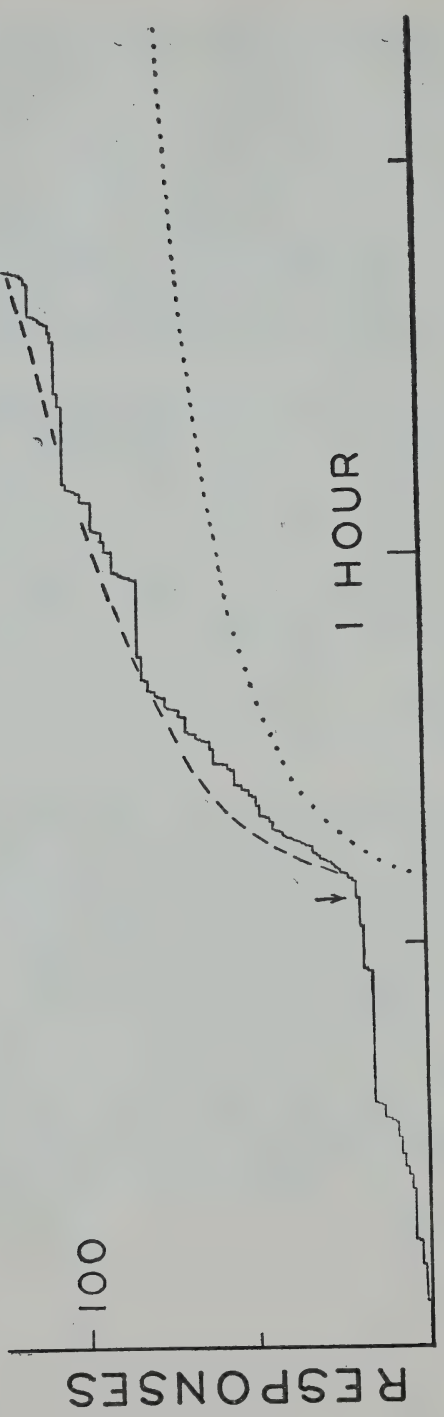

FIGURE 2

in Figure 2 is for the above equation with very nearly the same value of K. The broken line is for the same equation plus ct on the right-hand side, where c is another constant determined from the base-line. The figure thus compares very well with Figure 1, although the wave form is much less smoothly executed. Figure 2 also lacks the initial burst. The required shift in the origin would in this case be about two minutes. (It will be understood that the extinction curve begins at the first unreinforced response, not at the arrow.)

A probable explanation of the new base-line is as follows. In the preliminary training to the magazine a discrimination is established.[4] The rat comes to respond strongly to the food-tray-plus-sound-of-magazine, but not to respond to the food-tray alone. It can easily be shown that this discrimination is frequently very broadly generalized. The rat will respond to the tray plus almost any sort of extra stimulation. Now, even though the magazine remain disconnected, a response to the lever will stimulate the rat in several ways, and this stimulation—auditory, tactual, and kinaesthetic—may be sufficient to elicit the discriminatory response to the tray. Although no pellet is received and the sequence of reflexes therefore not eventually reinforced, the response to the lever and the discriminatory response to the tray are present, and some conditioning of the former should take place (2). This is what Pavlov has called secondary conditioning, although it should be noted that it is at least three steps removed from an unconditioned reflex.

According to this explanation, then, some conditioning may occur through generalization of the preliminary discrimination, without the reinforcement supplied by the sound of the magazine or the ultimate reinforcement with food. This view is supported by records for true secondary conditioning, where the sound of the magazine follows every response although no food is given (the magazine being empty). Figure 3 is a typical example of such an experiment. The rat had had the same preliminary training as in the other cases. On the day of the record the response to the lever was followed by the sound of the magazine and was therefore conditioned, but the response to the tray-plus-sound-of-magazine was simultaneously extinguished, since it was never followed by the delivery of food.[5] The result is a moderate rate of responding, which varies only very

[4]For the properties of a discrimination of this sort see (4).
[5]For discussions of extinction under these conditions see (3) and (5).

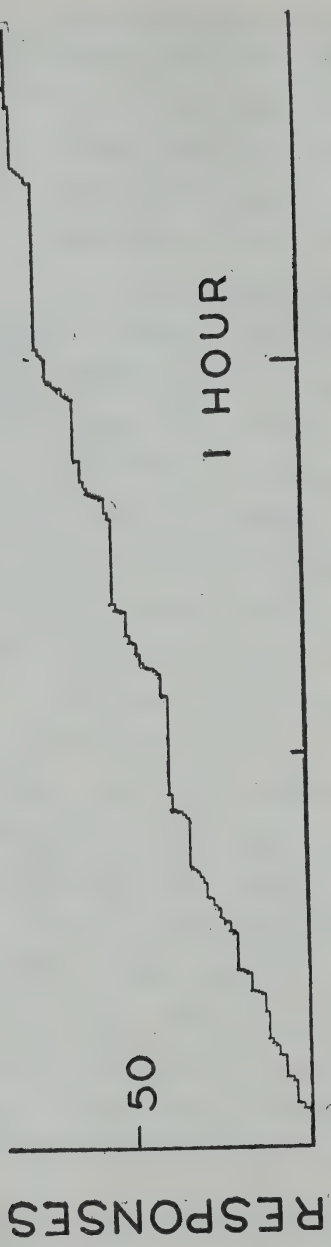

FIGURE 3

slightly during one and one-half hours, and which resembles in all essentials the modified base-line described above.

In the case of Figure 1 an adequate control against such an effect is provided by record *B*. Generalization of the discrimination has not occurred, and record *C* cannot be regarded as including a base-line of any significant slope. The curve is due wholly to the reinforcement of the first response and is not contributed to by partial reinforcement of other responses. This is obviously the simpler result, and some way should be devised to obtain it regularly. Although there is no insuperable difficulty in accounting for the effect of generalization, it is for certain purposes objectionable. The conditioning that occurs at the arrow in Figure 2, for example, is not *original* conditioning if our interpretation of the preceding base-line is correct.

It is evident that the extinction curve supplies considerable definite information about the change occurring at reinforcement. So far as resistance to extinction alone is concerned, some aspect of the curve (its height or area) is a proper measure of the *amount* of conditioning. In the present type of experiment, with a single reinforcement, this amount varies considerably among rats. The curves in Figures 1 and 2 are somewhat larger than the average, although they are by no means exceptional. In rare cases no effect may be detectable. "Good" records cannot, of course, be accidental. On the other hand, we have already noted several reasons why a first reinforcement may not be fully effective (2, p. 282). These sources of variability seem for the moment to be beyond our experimental control.

Summary

Two changes occur during the formation of a conditioned reflex: an increase in the immediately observed strength of the reflex, and an increase in its resistance to extinction. In many cases the latter is a preferable measure of the presence or absence of an effect.

The reinforcement of a single response may be followed by a significant curve of extinction. The curve serves as a measure of the amount of conditioning taking place. A complicating change sometimes observed in the rate of elicitation prior to specific reinforcement is described and discussed. The effect of a single reinforcement is still readily observable. In explaining the change, a record for secondary extinction (simultaneous conditioning and extinction) is described.

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"LA RÉSISTANCE FAITE À L'EXTINCTION" DANS LE PROCESSUS DE CONDITIONER

(Résumé)

Il se montre deux changements pendant la formation d'un réflexe conditionnel: un accroissement de la force immédiatement observée du réflexe, et un accroissement de la résistance qu'il fait à l'extinction. Dans plusieurs cas la dernière est une mesure préférable de la présence ou de l'absence d'un effet.

Le renforcement d'une seule réponse peut être suivi d'une courbe signifiante d'extinction. La courbe sert comme mesure de la quantité de conditionnement qui a lieu. On décrit et discute un changement de complication quelquefois observé dans la vitesse de l'apparence de la réponse avant un renforcement spécifique. L'effet d'un seul renforcement peut toujours être observé facilement. Dans l'explication du changement, on décrit une notation de l'extinction secondaire (le conditionnement et l'extinction en même temps).

Skinner

"WIDERSTAND GEGEN AUSLÖSCHUNG" IN DER ERSTELLUNG BEDINGTER REFLEXE

(Referat)

Zwei Veränderungen treten während der Erstellung eines bedingten Reflexes auf: eine Zunahme in der unmittelbar beobachteten Stärke des Reflexes und eine Zunahme des Widerstandes gegen die Auslöschung. In manchen Fällen ist dieser ein zu bevorzugendes Mass für das Auftreten oder Fehlen einer Wirkung.

Der Verstärkung einer einzelnen Reaktion kann eine bedeutsame Auslöschkurve folgen. Die Kurve dient als Mass der stattgefundenen Erstellung eines bedingten Reflexes. Die vorliegende Arbeit beschreibt und

diskutiert eine komplizierte Veränderung die gelegentlich in den Angaben einer Erstellung vor einer spezifischen Verstärkung zu beobachten ist. Die Wirkung einer einzelnen Verstärkung kann noch leicht beobachtet werden. Bei der Erklärung der Veränderung wird eine Aufzeichnung für sekundäre Auslöschung (simultane Erstellung bedingter Reflexe und Auslöschung) beschrieben.

SKINNER

DOES THE "COMPARE-AND-CONTRAST" ESSAY TEST MEASURE THE SAME MENTAL FUNCTIONS AS THE TRUE-FALSE TEST?*

From the Bureau of Educational Reference and Research, Teachers College, University of Nebraska

C. C. Weidemann and Lyndall Fisher Newens

I. Introduction

Paterson (5) and Wood (7, Chaps. 8-12) maintain that the new-type tests, such as the true-false, completion, multiple-choice, and matching types, measure the same mental functions as the old-type written essay test. Furthermore, they maintain that the consistency of the new types is at least as high as, and probably higher than, that of the old-type unimproved written essay test. In addition to the foregoing authorities, Ruch (6, pp. 70-111) and Odell (4, pp. 175-204) agree to this latter viewpoint.

In the claims of those cited, an essential point is that *improved* new-type tests were compared with *unimproved* essay tests. The purpose of this study is to determine the degree of overlapping of mental functions measured when one *improved* new-type test, namely, the true-false statement, is compared to one *improved* essay test, namely, the compare-and-contrast question. (See Section IV for examples.)

II. The Sample of Data

During the second semester, 1930-1931, in a required course, "The Foundations of Modern Education," taught by one instructor (Newens), a series of seven tests was given in each of four sections offered at 8 A. M., 9 A. M., 11 A. M., and 2 P. M., respectively. The personnel of the course consists mostly of sophomores, some juniors, a few seniors, and a small group of second-semester freshmen registered in the Teachers College, University of Nebraska. The total enrollment varied from 189 at the beginning to 179 at the close of

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the semester. Table 1 includes the enrollment in each section taking each test. It appears, then, that this study is a report of 28 experiments (seven tests for each of four sections).

III. General Procedures

The interval between each test of the series was two weeks. The same compare-and-contrast essay and true-false materials of a given test period were administered to each of the four sections. The test materials for each of the seven test periods differed from each other. Each of the seven units of test materials was based upon the content of the course of instruction over the two weeks immediately preceding the administering of that given unit.

An improved compare-and-contrast essay test, consisting of four questions, was given during one 50-minute class period. During the first 30 minutes of the succeeding class period, a true-false test, consisting of 80 items and covering approximately the same content of instruction, was administered. Throughout the study the same instructor, the same classroom for a given section, the same content of instruction for each section, and the same day of testing each section at the end of each two-week period were maintained. The health and the interest of the instructor in her work seemed consistently favorable. The instructor aimed to execute this study under actual classroom conditions. The general consistency of the results indicates that the study was executed under conditions favorable to the statement of conclusions, valid within the limits of the data.

All test results were read and scored by one student assistant who was experienced in the content of the course and under the direct supervision of the instructor of the course. Each essay question was read and scored for all sections before the reader proceeded to read and score the next essay question. An answer key was provided for each essay question as well as for each true-false test.

The essay tests were graded on the basis of 100, each question counting a possible maximum of 25 per cent. From zero to 20 per cent was allowed for the student's statement of minimum essentials prescribed by the instructor in the scoring key. Five per cent was used to grade acceptable items of additional content not included in the scoring key. The perfect score for the true-false test was 80. The method of scoring was the number right. Students were told not to omit items in either type of test.

IV. Instructional Directions to Students

At the opening of the semester, the instructor taught the students of each section how to formulate an answer to a compare-and-contrast essay question and how to answer the true-false tests.

A. The Compare-and-Contrast Essay Question

1. For the compare-and-contrast essay the student was told to *list* under (*a*) *comparison,* the similarities and dissimilarities of the two factors or events being considered, and under (*b*) *contrast,* the dissimilarities only of the two events being considered.

2. In order to *list* statements of comparison and contrast, it was assumed essential that a student must have mastered by recall the descriptive content of each of the two factors or events under consideration.

3. It was further assumed essential that each of the foregoing descriptions of the factors or events be based upon an outline prescribed by the instructor as a part of the content of the course.

4. Two samples of compare-and-contrast essay questions with their respective answers prescribed by the instructor follow:

Example 1: Compare the educational training of the youth of Sparta with that of the youth of Athens in the old Greek period.

SCORING KEY

1. Comparison of Aims
   a. Sparta—strength, courage, obedience to the laws.
   b. Athens—loyalty to state, but a more complete personal development allowed.
2. Comparison of Curricula
   a. Sparta—almost entirely physical and military.
   b. Athens—including physical and military elements, but a more liberal education emphasizing moral, intellectual, aesthetic elements as well.
3. Comparison of Methods
   a. Sparta—state control from birth.
   b. Athens—state supervision; education at state expense provided only for boys between the ages of 16 and 20.
   c. Sparta—mothers famous for care and training given children.
   d. Athens—slaves in charge of children.
4. Comparison of Results
   a. Sparta—produced warriors, possessing bravery, stoicism, strength and physical fitness, leaving nothing to the world but examples of heroism.

b. Athens—allowed individual development and a "liberal" education. Developed culture, art, literature, poetry and philosophy, in a city which became the seat of the intellectual world.

Examples of supplementary comparative content:

1. The life at the public barracks in Sparta.
2. The relationship of "hearer" and "inspirer."
3. The pedagogue.
4. Types of schools, palaestra and didascaleum.

Example 2: Contrast the Roman contribution to civilization with that of the Greeks.

SCORING KEY

1. Contributions of the Greeks:
   a. Greeks gave content.
   b. Greeks gave us the institution of the school.
   c. Made study of literature fundamental in the school.
   d. Supplied the beginnings of higher education.
   e. Supplied the world with a body of literature, philosophy, science to which we still turn.
   f. Created masterpieces in art, poetry, drama, sculpture, and architecture.
   g. With the Greeks, the apprenticeship form of education originated.
2. Contributions of the Romans:
   a. Romans gave organization and form.
   b. Maintained peace for Christianity.
   c. Created a body of law.
   d. Gave us the Latin language and literature.
   e. Roman architecture and buildings.
   f. Created idea of a universal empire.
   g. The Romans were weak where Greeks were strong and strong where the Greeks were weak. The Romans lacked imagination, speculation, but possessed the ability to embody Greek ideas in their institutions.

B. The True-False Tests

At the beginning of each of the seven true-false tests were the following directions:

1. If a statement is entirely *true,* put a plus (+) upon the space to the left of the statement.
2. If the statement is entirely *false,* put a zero (0) upon the space to the left of the statement.
3. If a statement is doubtful as to whether *true* or *false,* put the capital letter D upon the space to the left of the statement.
4. Do not omit any statement.

The instructor set the scoring key according to the authority of the text.

Occasionally an indeterminate statement was purposely inserted. Such a statement is doubtful as to whether true or false.

Four examples of true-false statements from the ancient systems of education with their answers keyed by the instructor follow:

\+ 3. State control in Sparta began with birth.
\+ 4. The Athenians allowed greater opportunities for individual development than did the Spartans.
0 5. The early Romans stressed art and literature in their education.
D 6. Roman education was compulsory and regulated by the government.

All of the compare-and-contrast essay questions, true-false, and indeterminate items were based upon the text by Graves (1).

V. Improvement in Students' Abilities to Answer the Compare-and-Contrast Essays and True-False Tests

That the instructional directions to the student were partially effective is evidenced (1) by observations made by the instructor, during the progress of the study throughout the semester, and (2) by supporting statistical data (see Table 1).

A digest of the seemingly significant observations made by the instructor during the tests and the classroom instructions follow:

1. Students improved in their ability to handle the compare-and-contrast tests.

2. Students improved in their ability to handle the true-false tests.

3. In the first true-false tests, students orally indicated difficulty in deciding upon indeterminate statements. Inspection indicated that for the first two true-false tests many definitely either true or false statements were answered as indeterminate. For the sixth and seventh true-false tests almost no statements were answered by the students as indeterminate.

4. Early in the semester students found difficulty in stating their answers to essay test questions in terms of comparisons and contrasts. After the first two tests students were thinking in these terms and answers to questions in class discussion indicated the same type of organization of material.

The data of Table 1 indicate that the students improved in their abilities to answer or to study for the compare-and-contrast essay. The means for the first test based on the four sections is 74 per cent;

TABLE 1

A Comparison of the Means and Standard Deviations of the Essay (Compare-Contrast) Examinations in Percentage Grade and True-False Examinations in Crude Scores for Each of the Four Sections in Each of the Seven Experiments

Section	Measures	Types of examination, number of the experiments: Essay (compare-contrast) in percentage 1	2	3	4	5	6	7	True-false in crude scores 1	2	3	4	5	6	7
I	M	74	77	84	84	81	86	87	63	63	62	66	68	62	67
	σ	20	15	11	14	14	11	9	5.2	6.6	5.8	5.7	5.5	6.8	4.7
II	M	74	81	86	85	80	85	83	61	64	65	66	68	63	66
	σ	14	12	8	12	13	12	14	6.4	5.0	5.3	5.0	5.7	5.3	4.3
III	M	76	83	86	88	87	86	90	63	64	66	65	69	62	67
	σ	14	9	10	9	12	13	7	5.7	6.2	5.2	5.4	4.6	5.5	4.0
IV	M	69	82	87	88	85	89	88	60	62	63	64	68	60	65
	σ	15	10	8	10	9	10	8	6.4	5.1	5.9	4.2	4.0	5.5	4.4
Median of means		74	81	86	87	83	86	87	62	63	64	65	68	62	67
Median of sigmas		15	11	9	11	13	11	9	6.1	5.7	5.5	5.2	5.5	5.5	4.3
Composite means		73.2						86.6	61.5						66.1
Composite standard deviations		16.5						10.6	6.4						4.5
Critical ratio of means					13.8							12.1			
Critical ratio of standard deviations					8.5							6.8			

that of the seventh test is 87. Similarly, the median of the standard deviations for the first based on the four sections is 15 per cent; that of the seventh test is 9. Based on 189 cases with a composite mean of 73.2 per cent and a composite standard deviation of 16.5 per cent for the first test, and on 172 cases with a composite mean of 86.6 per cent and a composite standard deviation of 10.6 per cent for the seventh test, the critical-ratio value of the differences of the foregoing means and standard deviation respectively are 13.8 and 8.5. The corresponding critical-ratio differences between the third test and the seventh test are respectively 1.8 and 1.9. The degree of improvement is greater between the first and third tests than between the third and seventh tests. The students of the four sections improved in their abilities to answer or to study for the compare-and-contrast essay questions between the first and seventh tests.

The data of Table 1 also indicate that the students improved in their abilities to answer the true-false tests. The median score of the means for the first test based on the four sections is 62; that of the seventh test is 67. Similarly, the median of the standard deviations for the first test based on the four sections is 6.1; that of the seventh test is 4.3.

Based on 189 cases with a composite mean score of 61.4 and a composite standard deviation score of 6.4 for the first test, and on 172 cases with a composite mean score of 66.1 and a composite standard deviation score of 4.5 for the seventh test, the critical-ratio value of the differences of the foregoing means and standard deviations respectively are 12.1 and 6.8. The corresponding critical-ratio differences between the third and the seventh tests are respectively 5.8 and 4.8. The corresponding critical-ratio differences between the fourth and seventh tests are respectively 2.0 and 2.8. The degree of improvement is greater between the first and fourth tests than between the fourth and seventh tests. The students of the four sections improved in their abilities to answer or to study for the true-false tests between the first and seventh tests, but not quite so quickly. The data of Table 1 agree substantially with the observations of the instructor.

VI. Treatment of Data

1. All original data were checked by the reader of the course.
2. All test scores and percentages were transcribed to data sheets and checked by reverse reading. All secondary transcription of data

sums were checked both by reverse reading and internal checking of cross-indexed sums so that the number of arithmetical errors was reduced to zero. An example of cross-indexed sums follows. Given 7, 4, 5, 9. Then not only 7 plus 4 are 11; 5 plus 9 are 14, and 11 plus 14 are 25; but also 7 plus 5 are 12; 4 plus 9 are 13, and 12 plus 13 are 25. Two final sum answers of 25 must check.

3. All calculations were executed and checked by a 20-place Monroe split-dial calculator and a 20-inch slide rule. When discrepancies of sums occurred, the data were checked until identical answers were secured. Calculations were carried to two decimal places of accuracy.

4. All transcriptions of results and typing of tables have been checked by reverse reading.

VII. Results of the Study

The phrase "community of function" is defined by Kelley (3, pp. 21, 193-196, 202-209) as the "proportion of the total variance which is due to the common factor present in each test." In relation to achievement and intelligence test scores, he says "that the coefficient of correlation between achievement and intelligence test scores corrected for attenuation is equal to the variance of the common factor divided by the total true variance."

This study is concerned with the community of function between the compare-and-contrast essay and the true-false tests. To what degree are the same mental functions measured by the compare-and-contrast essay and true-false tests as prepared and used throughout this study?

Table 2 presents the basic body of 105 consistency[1] coefficients from which community-of-function values may be determined. This table reveals certain interesting facts.

1. The median of 28 coefficients for the essay tests is 0.69; also for the true-false tests, the median is 0.69; and the median of the coefficients between the essay and true-false tests is 0.47.

2. The range of the 28 coefficients for the tests are for the essay, 0.40 to 0.84; for the true-false, 0.26 to 0.82; and for the essay with the true-false, 0.04 to 0.65.

3. The number of cases in Section III is too small for the Pear-

[1] "Consistency" as a descriptive word is preferred by the authors to the word "reliability."

TABLE 2

THE 105 CONSISTENCY COEFFICIENTS BETWEEN THE ODD-EVEN ESSAY (COMPARE-CONTRAST) ITEMS, BETWEEN THE ODD-EVEN TRUE-FALSE ITEMS, AND BETWEEN THE TOTAL ESSAY AND TOTAL TRUE-FALSE EXAMINATIONS FOR EACH OF THE FOUR SECTIONS AND FOR THE TOTAL GROUP IN EACH OF THE SEVEN EXPERIMENTS

Factors correlated	Number of the section	Number of the experiment 1		2		3		4		5		6		7	
		N	*r*	*N*	*r*	*N*	*r*	*N*	*r*	*N*	*r*	*N*	*r*	*N*	*r*
Essay items (odd, even) corrected by Brown's formula	I	54	.82	54	.82	51	.80	52	.58	49	.63	51	.65	51	.40
	II	56	.82	56	.82	54	.56	54	.65	52	.81	53	.52	53	.71
	III	32	.85	33	.50	32	.76	30	.76	33	.73	27	.84	27	.51
	IV	47	.67	47	.76	42	.67	44	.60	44	.69	41	.83	41	.57
	Total	189	.80	190	.78	179	.72	180	.60	178	.70	172	.67	172	.59
True-false items (odd-even) corrected by Brown's formula	I	54	.69	54	.73	53	.77	53	.82	52	.72	53	.77	53	.65
	II	56	.68	55	.66	50	.52	53	.73	54	.79	53	.73	53	.55
	III	32	.74	33	.79	31	.70	32	.72	32	.70	29	.51	29	.26
	IV	47	.75	47	.68	46	.56	44	.61	43	.50	44	.67	44	.66
	Total	189	.73	189	.69	180	.66	182	.73	181	.68	179	.71	179	.58
Essay items with true-false items	I	54	.55	54	.50	51	.65	52	.36	49	.48	51	.45	51	.26
	II	56	.50	55	.47	50	.33	52	.53	51	.35	53	.49	53	.46
	III	32	.52	33	.19	30	.29	30	.57	32	.50	27	.63	27	.24
	IV	47	.43	47	.31	41	.50	44	.27	42	.52	41	.04*	41	.23
	Total	189	.47	189	.40	172	.47	178	.39	174	.44	172	.37	172	.32

*This value was checked four times and found to be correct.

TABLE 3

THE 35 COMMUNITY-OF-FUNCTION COEFFICIENTS (*A*); THE 35 COMMUNITY-OF-FUNCTION COEFFICIENTS BASED ON CONSISTENCY COEFFICIENTS PLUS FOUR TIMES THEIR RESPECTIVE PROBABLE-ERROR VALUES (*B*); AND THE 35 COMMUNITY-OF-FUNCTION COEFFICIENTS BASED ON CONSISTENCY COEFFICIENTS MINUS FOUR TIMES THEIR RESPECTIVE PROBABLE-ERROR VALUES (*C*) FOR EACH OF THE FOUR SECTIONS AND FOR THE TOTAL GROUP IN EACH OF THE SEVEN EXPERIMENTS

Basis for computation of community-of-function values	Number of the section	Number of the experiment and community-of-function values: 1	2	3	4	5	6	7	Medians	
				A						
Consistency coefficients given in Table 2	I	.73	.65	.83	.52	.72	.63	.51	.65	
	II	.67	.65	.61	.77	.44	.79	.74	.67	
	III	.66	.30	.40	.77	.71	.97	.67	.67	
	IV	.61	.43	.82	.45	.88	.05	.38	.45	
	Total	.62	.55	.68	.59	.64	.54	.55	.59	
				B						
Consistency coefficients given in Table 2 plus 4 times their respective probable-error values	I	.86	.85	.92	.77	.87	.82	.79	.85	
	II	.86	.83	.86	.93	.73	.91	.87	.86	
	III	.93	.70	.77	.93	.95	1.02	.88	.93	
	IV	.84	.74	.94	.73	.99	.52	.72	.74	
	Total	.75	.67	.80	.72	.74	.70	.75	.74	
				C						
Consistency coefficients given in Table 2 minus 4 times their respective probable-error values	I	.52	.35	.70	.09	.43	.32	—.53	.35	
	II	.38	.33	—.10	.49	.04	.57	.45	.38	
	III	.25	—.83	—.29	.48	.27	.97	.00	.25	
	IV	.21	—.09	.51	—.26	.67	—.72	—.49	—.09	
	Total	.45	.38	.53	.41	.49	.30	.27	.41	
Median values	*A*	.67	.65	.71	.65	.71	.71	.59	.65	(28)*
	B	.86	.79	.89	.85	.91	.87	.83	.86	(28)*
	C	.31	.12	.21	.29	.35	.45	—.25	.29	(28)*

*Median based on 28 communition-of-function values.

son-*r* method of correlation; however, these coefficients compare favorably with the other values.

4. As the study progressed the following tendencies of change in size of the consistency coefficients based on the totals of the four sections are noted:

a. The essay values tend to decrease. These are 0.80, 0.78, 0.72, 0.60, 0.70, 0.67, and 0.59.

b. The true-false values show only a slight tendency to decrease. These are 0.73, 0.69, 0.66, 0.73, 0.68, 0.71, and 0.58.

c. The essay with true-false values tend to decrease. These are 0.47, 0.40, 0.47, 0.39, 0.44, 0.37, and 0.32.

Several factors may be responsible for the foregoing tendencies in both forms of tests: (1) the decreases in variability between Tests 1 and 7, (2) the increases in the means which cause a slight skew in the curve of distributions, and (3) a possible decrease in care of preparation of the tests as the study progressed.

Table 3 presents the community-of-function coefficients as described in its title. The typical overlapping of mental functions measured by this technique is approximately 60 per cent. The probable range of such overlapping would be approximately between 40 and 75 per cent. The median values given at the base of the table suggest that the foregoing range may be even greater and may extend from approximately 30 to 85 per cent.

Table 4 approaches the problem by combining the even, the odd, and all the tests of each form, and then computing the respective consistency coefficients. The following facts are interesting:

1. The consistency coefficients of the compare-and-contrast essay tests are practically as high as those of the true-false tests. In every case of the totals, the values are .80 or over. The unusual point is the high values for the compare-and-contrast essay.

2. The correlation of the essay tests and the true-false tests range between 0.61 and 0.71.

3. Of the original 189 students, 141 took every one of the seven essay and true-false tests given during the semester. This is 75-per-cent perfect attendance for the test periods.

Table 5 presents the community-of-function coefficients based on the values of Table 4. The typical overlapping of mental functions measured by this second technique is approximately 70 per cent. The probable range of such overlapping would be approximately between 55 and 83 per cent. The median values at the base of the

TABLE 4

THE 45 CONSISTENCY COEFFICIENTS BETWEEN THE ODD-EVEN ESSAY COMPARE-CONTRAST ITEMS, BETWEEN THE ODD-EVEN TRUE-FALSE ITEMS, AND BETWEEN THE TOTAL ESSAY AND TOTAL TRUE-FALSE EXAMINATIONS ON TESTS NUMBERED 2, 4, AND 6 COMBINED, 1, 3, 5, AND 7, COMBINED, AND 1 THROUGH 7 COMBINED FOR EACH OF THE FOUR SECTIONS AND FOR THE TOTAL GROUP

Factors correlated	Number of the section	Number	Groups of combined tests: 2, 4, and 6 *r*	1, 3, 5, and 7 *r*	1, 2, 3, 4, 5, 6, and 7 *r*
Essay items (odd, even) corrected by Brown's formula	I	45	.86	.90	.93
	II	41	.77	.88	.92
	III	22	.67	.63	.86
	IV	33	.79	.82	.91
	Total	141	.80	.86	.92
True-false items (odd, even) corrected by Brown's formula	I	45	.89	.94	.89
	II	41	.85	.96	.88
	III	22	.67	.74	.92
	IV	33	.80	.82	.89
	Total	141	.84	.92	.89
Essay items with true-false items	I	45	.82	.70	.74
	II	41	.71	.67	.63
	III	22	.63	.36	.23
	IV	33	.66	.53	.57
	Total	141	.71	.61	.62

TABLE 5

THE 15 COMMUNITY-OF-FUNCTION COEFFICIENTS (*A*); THE 15 COMMUNITY-OF-FUNCTION COEFFICIENTS BASED ON CONSISTENCY COEFFICIENTS PLUS FOUR TIMES THEIR RESPECTIVE PROBABLE-ERROR VALUES (*B*); AND THE 15 COMMUNITY-OF-FUNCTION COEFFICIENTS BASED ON CONSISTENCY COEFFICIENTS MINUS FOUR TIMES THEIR RESPECTIVE PROBABLE-ERROR VALUES (*C*) FOR TESTS NUMBERED 2, 4, AND 6 COMBINED, 1, 3, 5, AND 7 COMBINED, AND 1 THROUGH 7 COMBINED FOR EACH OF THE FOUR SECTIONS AND FOR THE TOTAL GROUP

Basis for computation of community-of-function values	Number of the section	Groups of combined tests and community-of-function values: 2, 4, and 6	1, 3, 5, and 7	1, 2, 3, 4, 5, 6, and 7	Medians
		A			
Consistency coefficients given in Table 4	I	.94	.76	.81	.81
	II	.88	.73	.70	.73
	III	.94	.53	.26	.53
	IV	.83	.65	.63	.65
	Total	.87	.69	.69	.69
		B			
Consistency coefficients given in Table 4 plus 4 times their respective probable-error values	I	.97	.92	.97	.97
	II	.96	.93	.89	.93
	III	.98	.89	.75	.89
	IV	.96	.89	.91	.91
	Total	.91	.82	.83	.83
		C			
Consistency coefficients given in Table 4 minus 4 times their respective probable-error values	I	.89	.58	.64	.64
	II	.76	.50	.47	.50
	III	.83	—.44	—.38	—.38
	IV	.63	.29	.30	.30
	Total	.81	.54	.53	.54
Median values	*A*	.91	.69	.67	.75 (12)*
	B	.97	.91	.90	.93 (12)*
	C	.79	.39	.39	.54 (12)*

*Median based on 12 community-of-function values.

TABLE 6

THE 105 CONSISTENCY COEFFICIENTS OF TABLE 2 CORRECTED BY THE FORMULA $r_{1\infty} = \sqrt{r_{1I}}$ (2, p. 206), THE CORRELATION BETWEEN ONE FORM OF A TEST AND A TRUE SCORE OF THE FUNCTION MEASURED BY THE TEST

Correlated factors corrected	Number of the section	Number of the experiment 1	2	3	4	5	6	7
Essay items (odd, even)	I	.91	.91	.89	.76	.79	.81	.63
corrected by	II	.91	.91	.75	.81	.90	.72	.84
Kelley's	III	.92	.71	.87	.87	.85	.92	.71
$\sqrt{r_{1I}}$	IV	.82	.87	.82	.78	.83	.91	.76
	Total	.89	.88	.85	.77	.84	.82	.77
True-false items (odd, even) corrected by	I	.83	.85	.88	.91	.85	.88	.81
	II	.83	.81	.72	.85	.89	.86	.74
Kelley's	III	.86	.89	.84	.85	.84	.71	.51
$\sqrt{r_{1I}}$	IV	.87	.83	.75	.78	.71	.82	.81
	Total	.85	.83	.81	.85	.82	.84	.76
Essay items with true-false items	I	.74	.71	.81	.60	.69	.67	.51
corrected by	II	.71	.69	.57	.73	.59	.70	.68
Kelley's	III	.72	.44	.54	.75	.71	.79	.49
$\sqrt{r_{1I}}$	IV	.66	.56	.71	.52	.72	.20	.48
	Total	.69	.63	.69	.62	.66	.61	.57

table suggest that the foregoing range may extend from approximately 55 to 93 per cent.

Table 6 presents the 105 coefficients of Table 2 corrected by the formula, $r_{1\infty} = \sqrt{r_{1I}}$ (2, p. 206). These corrected values represent the maximum which any coefficient of Table 2 may ever become. The values of Table 6 are used as a means of computing the community-of-function values of Table 7.

Table 7 indicates that the typical degree of overlapping between the compare-and-contrast essay and true-false tests when one form of a test is correlated with a true score of the function measured by the test is approximately 80 per cent. The range of the typical (median) values is approximately between 65 and 85 per cent.

Table 8 presents the 45 coefficients of Table 4 corrected by the formula, $r_{1\infty} = \sqrt{r_{1I}}$ (2, p. 206). This corrected set of values represents the maximum which any coefficient of Table 4 may ever become. The values of Table 8 are used as a means of computing the community-of-function values of Table 9.

Table 9 indicates that the typical degree of overlapping of mental functions measured between the compare-and-contrast essay and the true-false tests when one form is correlated with a true score of the function measured by the test is approximately 85 per cent. The range of the typical (median) values is approximately between 73 and 95 per cent.

By the four foregoing techniques, the typical values for the overlapping of mental functions measured by the compare-and-contrast essay and true-false tests as prepared and used in this study are:

1. Classroom conditions, test by test, section by section, approximately 60 per cent.
2. Classroom conditions, combinations of tests (even, odd, and all), section by section, approximately 70 per cent.
3. Classroom conditions, test by test, with maximum validity of mental functions measured assumed, section by section, approximately 80 per cent.
4. Classroom conditions, combinations of tests (even, odd, and all) with maximum validity of mental functions measured assumed, section by section, approximately 85 per cent.

These results indicate that under no conditions do the compare-and-contrast essay tests and true-false tests as prepared and used in

TABLE 7

THE 35 COMMUNITY-OF-FUNCTION COEFFICIENTS BASED ON THE CORRECTED VALUES OF TABLE 6 FOR SEVEN EXPERIMENTS IN EACH OF FOUR SECTIONS AND FOR THE TOTAL GROUP, AND MEDIANS

Number of the section	Number of the experiment							Medians
	1	2	3	4	5	6	7	
I	.85	.81	.91	.72	.84	.79	.71	.81
II	.82	.80	.78	.88	.66	.89	.86	.82
III	.81	.55	.63	.87	.84	.98	.81	.81
IV	.78	.66	.90	.64	.94	.23	.61	.66
Total	.78	.74	.83	.77	.80	.73	.74	.75
Medians	.81	.71	.84	.79	.84	.84	.76	.81 (28)*

*Median based on 28 community-of-function values.

TABLE 8

THE 45 CONSISTENCY COEFFICIENTS OF TABLE 4 CORRECTED BY THE FORMULA $r_{1\infty} = \sqrt{r_{11}}$ (2, p. 206)

Correlated factors corrected	Number of the section	Groups of combined tests: 2, 4, and 6 r	1, 3, 5, and 7 r	1, 2, 3, 4, 5, 6, and 7 r
Essay items	I	.93	.95	.96
	II	.88	.94	.96
	III	.82	.79	.93
	IV	.89	.91	.95
	Total	.89	.93	.96
True-false items	I	.94	.97	.94
	II	.92	.98	.94
	III	.82	.86	.96
	IV	.89	.91	.94
	Total	.92	.96	.94
Essay items with true-false items	I	.91	.84	.86
	II	.84	.82	.79
	III	.79	.60	.48
	IV	.81	.73	.76
	Total	.84	.78	.79

TABLE 9

THE 15 COMMUNITY-OF-FUNCTION COEFFICIENTS BASED ON THE CORRECTED VALUES OF TABLE 8 FOR THE COMBINED EVEN, ODD, AND ALL TESTS OF THE STUDY

Number of the section	Groups of combined tests			
	2, 4, and 6	1, 3, 5, and 7	1, 2, 3, 4, 5, 6, and 7	Medians
I	.97	.87	.90	.90
II	.93	.85	.83	.85
III	.96	.73	.51	.73
IV	.91	.80	.80	.80
Total	.93	.83	.83	.83
Median	.95	.83	.81	.86 (12)*

*Median based on 12 community-of-function values.

this study measure identical mental functions to the extent of 100 per cent.

Approximately 40 per cent of the mental functions measured by the compare-and-contrast essay tests are not measured by the true-false tests.

Approximately 40 per cent of the mental functions measured by the true-false tests are not measured by the compare-and-contrast essay tests.

Approximately 60 per cent of the mental functions measured by the compare-and-contrast essay tests and the true-false tests are identical.

VIII. TENTATIVE CONCLUSIONS AND THOUGHTS FOR FURTHER STUDY

1. During the progress of the study, students improved in their abilities to answer both the compare-and-contrast essay and true-false tests.

2. As prepared and used in this study, the consistency of the compare-and-contrast essay tests is as high as the consistency of the true-false tests. In no case are the differences in the respective consistency coefficients based on the total number of cases either significant or distinguishable for classroom purposes.

3. So far as this study is concerned, the compare-and-contrast essay and true-false tests under actual classroom conditions of ad-

ministration overlap in mental functions measured to the extent of the approximate median values of 60 to 70 per cent.

4. When one form of the test is correlated with the true scores of the function measured by the test, the degree of overlapping of mental functions measured increased to the approximate median values of 80 and 85 per cent.

5. From approximately 30 to 40 per cent of the mental functions measured by the compare-and-contrast essay tests are different from another 30 to 40 per cent of the mental functions measured by the true-false tests as prepared and used under the actual classroom conditions of this study.

6. This study opens a rich field for further investigation involving the long list of studies on "community of function" between every carefully prepared form, type, and variety of both objective and essay forms for all subjects, their combinations and permutations.

7. An important question evolving from this study is what is the nature of each of the mental functions measured by each variety of test form?

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L'ESSAI "COMPARAISON ET CONTRASTE" MÉSURE-T-ILS LES MÊMES FONCTIONS MENTALES QUE LE TEST VRAI-FAUX?

(Résumé)

Le titre indique le but de l'étude. Pendant un semestre, sept tests ont été donnés à 179 étudiants divisés en quatre sections du même cours fait par le même professeur (Newens). On a employé toujours les mêmes

méthodes générales d'enseigner et de noter les résultats. Chaque test d'essai s'est composé de 4 questions; chaque test vrai-faux, de 80 parties.

Les donées n'indiquent aucun accroissement de l'habileté moyenne à répondre à l'un ou à l'autre type des tests entre la quatrième période d'épreuve et la septième y compris. L'accroissement est signifiant entre la première période et la troisième.

Les conclusions limitées à l'étude suivent:

1. La constance des deux types de test est environ la même.
2. Les deux types de test montrent dans les fonctions mentales mesurées un empiétement de soixante à soixante-dix pour cent.
3. Trente à quarante pour cent des fonctions mentales mesurées par les tests d'essai ne sont pas mesurées par les tests vrai-faux.
4. Trente à quarante pour cent des fonctions mentales mesurées par les tests vrai-faux ne sont pas mesurées par les tests d'essai.

On devrait faire un grand nombre d'études pareilles avec d'autres combinaisons de types de test. Cette étude suggère d'ailleurs la question, "Quelle est la nature de chacune des fonctions mentales mesurée par chaque type de test?"

WEIDEMANN ET NEWENS

MISST DER "VERGLEICH UND KONTRAST" ESSAY DIE GLEICHEN GEISTESFUNKTIONEN DIE DER WAHR-FALSCH TEST?

(Referat)

Die Aufgabe dieser Untersuchung ist im Titel angezeigt. Während eines Semesters gab man sieben Teste 179 Studenten, die vier verschiedenen Sektionen einer Klasse angehörten, und von demselben Lehrer unterrichtet wurden (Newens). Es wurden immer dieselben Lehrmethoden und Testmethoden angewandt. Jeder Essay bestand aus vier Fragen; jeder Wahr-falsch Test aus 80 Punkten.

Die Angaben zeigen zwischen der vierten bis siebenten Testperiode keine zunahme der durchschnittlichen Fähigkeit als Folge eines der beiden Teste. Die Zunahme ist bedeutsam zwischen der ersten und dritten Testperiode.

Die auf die Versuche gestützten Folgerungen sind:

1. Die Beständigkeit der beiden Testtypen ist ungefähr gleich.
2. Die Geistesfunktionen, die durch die beiden Teste gemessen werden, decken sich im Umfange von sechzig bis seibzig Prozent.
3. Dreissig bis vierzig Prozent der Geistesfunktionen, die durch den Essaytest gemessen werden, werden nicht durch den Wahr-falsch Test gemessen.
4. Drissig bis vierzig Prozent der Geistesfunktionen, die durch den Wahr-falsch Test gemessen werden, werden nicht durch den Essaytest gemessen.

Es sollten viele andere ähuliche Untersuchungen mit anderen Kombinationen von Testtypen vorgenommen werden. Diese Untersuchung führt ferner zur Frage: "Was ist die Natur jener Geistesfunktionen, die durch jede der Testformen gemessen werden?"

WEIDEMANN UND NEWENS

SHORT ARTICLES AND NOTES

COLOR CONTRAST OF THE SECOND ORDER*

C. E. Ferree and G. Rand

In former papers (1, 2) we have shown that a contrast excitation may give rise to an after-image. It is the purpose of the present paper to show that it may also induce a second contrast excitation.

The demonstration of the phenomenon has formed a part of our teaching work for many years. It was obtained under the following conditions. The special sensitizing condition was a quick change from medium high to low illumination. The contrast is observed while the illumination is changing.

The experiment was conducted in a room 15 feet long, 8 feet wide, and 10 feet high. The room was light-proofed for a dark room but the walls were of rough unpainted natural plaster. At one end of the room was a window 3 feet high and 2 feet wide. This window was provided with a heavy light-proofed door or shutter. The bottom of this window was about 5.5 feet from the floor. By closing the window at a suitable rate, a quick uniform change in illumination could be produced. A light-proofed curtain is not so suitable for the purpose. With such a curtain it is not easy to get the optimum rate of change in the illumination and it cannot easily be made so uniformly as with the door noted above. Daylight illumination is required. In front of this window, and facing it, was mounted a rotary color mixer. On it were placed the following discs: an outside disc of color with a radius of 13.5 cm., and an inside disc of the same color with a radius of 6.25 cm. Between these were three pairs of discs of white and black, one pair of which had a radius of 9.4 cm., one a radius of 8.35 cm., and the third a radius of 7.3 cm. When these black and white discs were adjusted in proper proportion and the entire combination was rotated, the surface presented consisted of an outer rim of color 4.1 cm. wide; an inner disc of color with a radius of 6.25 cm., and three gray rings between them, each 10.5 mm. wide. The purpose of the outer and the inner gray rings was to receive the induction from the neighboring colored surfaces and to act in turn as inducing surfaces for the middle ring. The purpose of the middle ring was, as indicated, to receive the secondary induction. The proportions of white and black in the outer and inner rings were adjusted to give a gray of the brightness of the inducing color; the proportion in the middle ring, to give a gray of approximately the brightness of the induced color.

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For example, if the inducing color was the standard red of the Hering series, the outer and inner rings were made of the brightness of this red and the middle ring was made approximately the brightness of the standard green. The middle ring was made of the approximate brightness of the induced color because (*a*) it must differ in brightness from the two outer rings to permit the induced color on these rings from spreading to it as well, and (*b*) a gray of the brightness of the induced color is favorable to the saturation of that color. In every case, however, the optimum brightness of this ring has to be determined empirically. When these discs are viewed either at high or low illumination, scarcely any contrast is present even in the outer rings, because there is color only on one side of them; and none at all in the inner ring. If the observation is made while the illumination is changing, however, the outer and inner rings are seen in the color of the complement to the inducing color and the middle ring in the hue which is the complement of that, or in the same hue as the original or primary inducing color. For the greater number of observers we have used in twenty or more years the best results have been obtained with red as inducing color. The effect may be produced however with any well-saturated color.

Another way in which our method of sensitizing the induction of contrast is of service is in showing more strikingly that color induced as contrast mixes with and modifies color sensations aroused directly by light stimulation just as effectively as if it were also aroused directly by the action of light. For example, if a test field is made up of two discs of blue with radii respectively of 12 cm. and 5 cm. and a third disc of red with a radius of 6 cm., presenting a surface comprising an outer rim and an inner disc of blue with a ring of red between, some modification of the red is produced by the induced yellow when the observation is made with a static illumination either high or low. If, on the other hand, the observation is made while the illumination is changing from high to low, the red ring flashes out as a rich, brilliant orange. This observation has a bearing, of course, on theories of contrast. It shows perhaps more strikingly than has ever been shown in any other way that contrast behaves as if it were a peripherally aroused excitation and not an illusion based on any association of ideas. The very fact that so great an increase of contrast can be observed while the illumination is changing might also be cited as an argument that contrast is due to a peripherally aroused excitation; i.e., it might be contended that this increase is more apt to be conditioned by the interaction of purely sensory processes than by an influence of non-sensory factors. On the other hand, however, it might be argued that clear vision is confused while the illumination is changing and that illusory effects are favored thereby. The theorist has ever a comforting range of possibilities.

A third use of the method of sensitizing contrast will be discussed in a later paper. Success in using the method depends, it must be remembered, on being able to secure the optimum control of the change in the intensity of illumination. Control by curtains is not recommended. It is also peculiarly dependent on the use of daylight. Artificial light seems to be inimical to both contrast and after-image phenomena. In this, diffusion and direction of light may be a factor, as well as color. The state of adaptation in which the eye ordinarily is when daylight is used may also in some cases have an influence.

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THE GEOTROPIC CONDUCT OF YOUNG KITTENS: II

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In a recent experiment (2) upon the geotropic conduct of young kittens it was found that the magnitude of the angle of orientation upon an inclined plane varied with the slope of the plane. The angle of orientation, θ, was found to be very nearly proportional to the logarithm of the sin of the angle of inclination, α. In this experiment the values of θ were obtained at slopes of 30 degrees and above. The resulting curve seemed by comparison with that known for other mammals (1, 3) to be the top part of a sigmoid curve.

The present experiment was made (1) to secure if possible values of θ at several slopes below 30 degrees; (2) to control the age factor, a variable which was not sufficiently controlled in the first experiment; and (3) to discover whether the minimal usable threshold slope varied in any way with the age of the animal.

The apparatus used in this experiment has been fully discussed in the first paper (1) and need not be described here. The first part of this experiment was conducted within a period of 72 hours. The first angles of orientation were recorded 15 hours after the birth of the animals. Due to the fact that the kittens tired very rapidly, only 20 trails were recorded for the group at each angle of inclination. Three kittens (littermates) were used.

Table 1 presents the data.

TABLE 1

$N = 3$ (number of individuals), $n = 20$ [*total* number of trails, for the three individuals at each slope (α)].

α degrees	Mean θ degrees	P.E. degrees
15	45.70	1.76
25	57.35	1.58
30	61.80	0.93
40	76.90	0.46
50	84.20	0.23
60	86.05	0.15
65	87.35	0.16

Figure 1 shows the relation of θ, the mean angle of orientation, to log sin α. The curve is sigmoid in character and supports the assumption made in the first experiment (2), where it was suggested that the curve (showing the relationship between θ and log sin α) was really the top part of an S-shaped curve. In that experiment it was impossible to make observations at the lower slopes because the animals had grown too old.

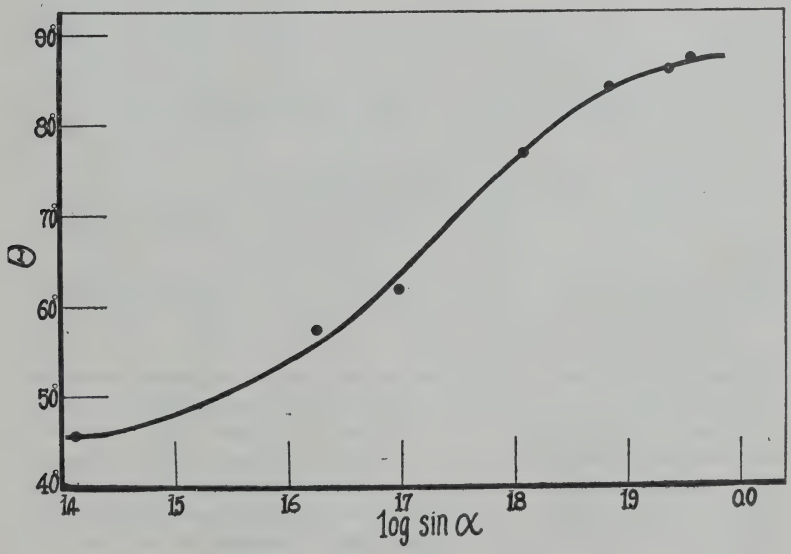

FIGURE 1

θ as a function of log sin α. Each point is the mean of 20 trails.

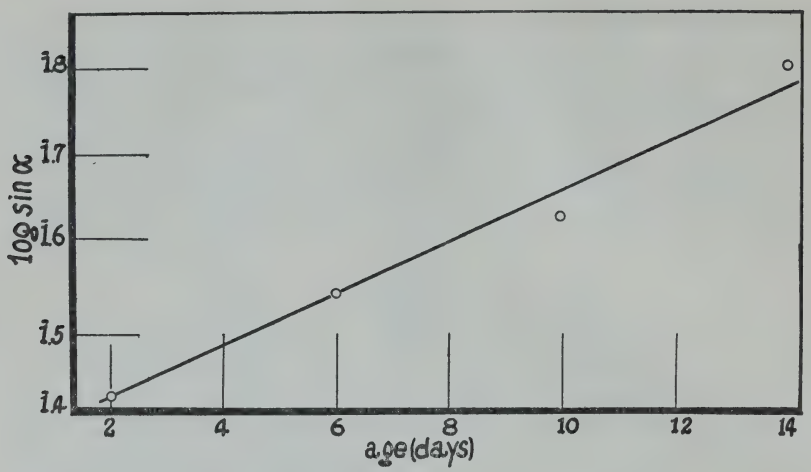

FIGURE 2

The minimal usable threshold slope (α) increases with the age of the animal. The logarithm of the sin of the angle of inclination (α) increases rectilinearly with an increase in age.

The variability of mean θ (expressed by the P.E. of mean θ) decreases lawfully as the gravitational stimulus is increased. The P.E. of mean θ decreases proportionally with an increase in sin α. As in experiment 1 (2), the P. E. of mean θ and θ are inversely correlated, indicating that θ and the variability of θ are controlled by the same mechanism. This relationship between the P.E. of mean θ and θ has been found to hold true of a number of other forms that have been tested (1, 3).

Experiments on rats (1) have shown that the specific mean magnitudes of θ are repeatedly recoverable for different groups of the same strain. With kittens no such conclusions can as yet be drawn, but it is interesting to point out that a comparison of the data of the two groups tested in these experiments reveals in general a similarity indicating that this may hold for genetically like strains of kittens.

Because no orientation was gotten for the kittens in the first experiment for slopes below 30 degrees, it was thought desirable to discover whether the minimal threshold slope varied as a function of age. This was done in the following manner: The animals were tested at intervals of 4 days, first at the inclination which had proved to be the last minimal threshold, and then, if no orientation was evident, the animal was tested at a slope (α) increased by 5 degrees. Figure 2 shows the relationship between log sin α (α representing the minimal threshold slope) and the age of the animal (in days). It is evident that the minimal usable threshold does alter with the

age of the animal. At 6 days of age, orientation could not be obtained at $\alpha = 15$ degrees; at 10 days the threshold had moved up to 25 degrees; and at 14 days geonegative behavior was evident only at a minimal slope of 40 degrees. At angles below the minimal threshold slope the animals either slipped down or turned on the plane and crept down.

Summary

Kittens progressing upward upon an inclined plane orient at an angle θ which is a function of the slope of the plane. Equations describing this behavior show that θ is very nearly proportional to log sin α. Cos. θ plotted against sin α gives a rectilinear relationship. The variability of θ decreases as the slope of the plane is increased. The minimal usable threshold slope increases as a function of the age of the animal.

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VISUAL DOMINANCE—ACUITY RELATIONSHIPS

Lawrence Gahagan

Determinations of ocular dominance and visual acuity were made upon one hundred persons to discover whether or not a relationship exists between these two visual conditions. It is well known that many individuals possess unequal visual acuity of the two eyes. In such cases, maximal visual efficiency obviously exists in the preferential use of the eye with the greater acuity. The aim of this investigation was to discover how frequently a unilateral dominance is related to visual acuity.

Forty of the subjects were young men and sixty were young women; all but three were undergraduate university students. Persons who wore glasses habitually were not used. Hence, the subjects as a whole might be supposed to possess acuity above the average of the population.

Ocular dominance was determined by having the subject sight at a small object through a hole in a piece of cardboard.[1] Subjects were tested both for near fixation (about 1 meter) and distant fixation (about 15 meters).

[1]Much as described by Scheidemann (3).

Only those who showed definite and stable ocular dominance were retained in the experiment. Visual acuity was determined by means of the McCallie vision test for literates.

The results of the tests are shown in Tables 1 and 2. In the first column are the designations of the groups showing the relative visual acuities and in the second column are the associated ocular dominances. In the third column there is shown the number of cases of each kind of dominance—acuity relationship.

All discovered inequalities in visual acuity of the two eyes are listed in Table 1, regardless of the amount of the difference. For example, a subject who showed a right-eye acuity of 28/20 and a left-eye acuity of 30/20 is listed in Table 1 as possessing greater visual acuity of the left eye. In

TABLE 1

SHOWING THE RELATIONSHIPS EXISTING BETWEEN VISUAL ACUITY AND OCULAR DOMINANCE

Relative acuities	Dominant eye	Number of cases
Right eye+	Right	34
	Left	8
Left eye +	Right	29
	Left	13
Right and left eyes =	Right	16
	Left	0
	Total	100

TABLE 2

SHOWING THE RELATIONSHIPS BETWEEN OCULAR DOMINANCE AND VISUAL ACUITY IN WHICH THE EYES DIFFER BY MORE THAN ONE-TENTH NORMAL VISION

Relative acuities	Dominant eye	Number of cases
Right eye+	Right	26
	Left	7
	Total	33
Left eye +	Right	20
	Left	7
	Total	27
Right and left eyes =	Right	33
	Left	7
	Total	40
	Total	100

Table 2, however, only those inequalities which exceed one-tenth normal vision are listed as inequalities. Smaller differences, of one-tenth normal vision or less, are dealt with as equal acuities of the two eyes. Because of this, the person noted above who is listed as an "inequality" in Table 1, is listed in Table 2 as "equal acuity." Table 2 more adequately repre-

TABLE 3

TWENTY CASES IN WHICH THE RIGHT EYE IS DOMINANT AND THE LEFT EYE POSSESSES GREATER ACUITY

Number	Right-eye acuity	Left-eye acuity	Differences amounts	Percentage L. eye / R. eye
1	52	67	15	129
2	70	90	20	129
3	80	100	20	125
4	90	110	20	122
5	110	130	20	118
6	120	140	20	117
7	75	100	25	133
8	100	130	30	130
9	110	140	30	127
10	140	170	30	121
11	150	180	30	120
12	150	180	30	120
13	67	100	33	149
14	50	90	40	180
15	90	130	40	144
16	24	70	46	292
17	100	150	50	150
18	80	133	53	166
19	90	150	60	167
20	130	190	60	146

SEVEN CASES IN WHICH THE LEFT EYE IS DOMINANT AND THE RIGHT EYE POSSESSES GREATER ACUITY

				R. eye / L. eye
21	140	120	20	117
22	150	130	20	115
23	110	87	23	126
24	150	120	30	125
25	150	120	30	125
26	140	100	40	140
27	150	100	50	150

sents the result freed from probable accidental errors, because it disregards minor and fluctuating differences in acuity.

On the basis of the determinations, 79 of the 100 subjects were found to be right-eyed, and 21 left-eyed. This is a higher proportion of right-eyedness than is usually found. Miles (1), in summarizing his work and that of others, reports that a population normally shows a proportion of about 65 per cent right-eyed, 30 per cent left-eyed, and 5 per cent indefinite. In this experiment, individuals of indefinite dominance were not used.

Table 2 also shows that 60 subjects had unequal visual acuity of the two eyes, in contrast to 40 who showed equal acuity. Of this group of unequal acuity, 55 per cent had the greater acuity of the right eye, and 45 per cent had greater acuity of the left eye. These proportions of superiority of the right and left eye are not significantly different.

From this table it is also seen that superior visual acuity of a given eye is not indicative of a corresponding dominance of that eye. Of the 33 cases with a superior visual acuity of the right eye, 26 showed right-eye dominance, whereas of the 27 cases possessing greater acuity of the left eye only 7 showed left-eye dominance. Thus, in the cases of unequal visual acuity, 55 per cent showed the condition of maximal efficiency, viz., agreement of acuity and dominance. This is contrary to the opinion sometimes held that acuity indicates dominance (1, p. 415). Because the 27 cases of minimal visual efficiency, cases of disagreement of dominance and acuity, are the most interesting, they are individually listed in Table 3. In the listings are given the acuity of the right eye, the acuity of the left eye, and the differences of the two acuities. Acuities are indicated in terms of percentage of normal vision; for example, visual acuity 22/20 is given as 110 (percentage of normal vision), 12/50 as 24, etc. Inspection of Table 3 shows that some individuals have a pronounced loss of visual efficiency on account of the lack of dominance-acuity correspondence. The outstanding example is Case 16, which concerns a right-eye acuity of 12/50 and a left-eye acuity of 14/20. A change in dominance from the right to the left eye would more than double the visual efficiency, as measured by the dominance-acuity relationship. Cases 15, 18, 19, and 23 are also worthy of attention inasmuch as they show the dominant eye to have less than normal acuity and the non-dominant eye to be better than the average or normal. Four other cases (3, 4, 7, and 13) have the non-dominant eye normal and the dominant eye subnormal.

Summary

Dominance and acuity are here shown to be independent visual phenomena. A fairly large proportion of the subjects, 27 per cent, showed a condition of minimal visual efficiency with respect to the two variables, as opposed to 33 per cent who showed conditions of maximal efficiency. The remaining

40 per cent of the subjects showed approximately equal visual acuity of the two eyes, and with them the question of efficiency in the dominance-acuity relationship does not arise.

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A POSSIBLE EXPERIMENTAL ERROR IN DETERMINING THE OVERLAP OF TWO CORRELATED VARIABLES

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In determining the true overlapping of two correlated variables, as, for example, two mental functions as defined by two different measuring instruments, the usual procedure is to determine the reliability coefficients of the two measuring instruments, and from these data to determine the "true" relationship between the two variables in question in terms of r corrected for attenuation. Stated as a formula, the above procedure is

$$r_{\infty\infty} = \frac{r_{12}}{\sqrt{r_{11}}\sqrt{r_{22}}} \qquad \text{(2, pp. 204-205)}$$

The purpose of this paper is to point out a systematic experimental error in certain aspects of the procedure as frequently applied, particularly with reference to the measurement of overlap of mental functions. The effect of this error is to yield a value of $r_{\infty\infty}$ which is spuriously low.

The error lies in the methods used in the experimental determination or r_{11} and r_{22}.

As is well known, there are in general three methods of determining the reliability or "self-correlation" of a measure:

1. The "split-test" method, in which the test is rescored in such a way as to obtain scores on two chance halves of the test. These two series of scores are then correlated and "stepped up" by the Spearman-Brown prophecy formula to obtain the reliability of the whole test.

2. Administering the same test at two different times and correlating the two series of scores obtained.

3. Administering two equivalent forms of the test successively and correlating the scores obtained.

Methods 1 and 3 are the most defensible and the most widely used. It is evident that these two methods differ chiefly in that in the first case—assuming the validity of the Spearman-Brown formula—a pool of test items is made into two equivalent forms *after* administering the entire pool to the subjects measured, while in the second case this is done *before* the testing.

It is of crucial importance to note, however, that these two methods do not, in general, yield the same reliability coefficients. The reliability obtained by the "split-test" method will in general be higher than that obtained from the "equivalent-forms" method. In the "split-test" method the factors which operate during the testing to make the subjects different at one time than at another and thus influence the reliability adversely are usually very much minimized as compared with their effect on the "equivalent-forms" technique. Such factors as fatigue, monotony, distractions, etc., will be much more likely to affect reliability adversely in the latter technique, because the two test forms are given successively, while in the "split-test" technique the two "forms" are administered, so to say, at the same time, thus controlling much better the factors mentioned.

It is at once obvious, therefore, that in the measurement of overlapping of mental functions by the correlation method, $r_{\infty\infty}$ will yield a lower value when the "split-test" technique is employed than would be the case if the same test items had been made into two chance halves in advance of the measurement and the two forms administered successively.

To illustrate, suppose that it is desired to determine the extent to which "ability to reason in terms of subject-matter taught" is measured by two different measuring instruments, such as a written discussion examination and an objectively scored, or "new type," examination. Let the discussion examination be the criterion of the ability to reason, it having been graded for this ability alone. Let the obtained values for r_{12}, $r_{1\text{II}}$, and $r_{2\text{II}}$ be as follows:

	Reliability	
	Split-test method	Equivalent-forms method
1. Discussion	.60	.48
2. Objective test	.92	.75

$r_{12} = .58$

Then $r_{\infty\infty}$ for the "equivalent-forms" procedure would equal .97, while for the "split-test" procedure it would equal .78.

The conclusions to be drawn in the two cases would differ rather radically. With a reasonably large population from which the r's were determined one would conclude that the deviation of .97 from 1.00, or perfect correlation, and complete overlapping was easily within the allowable sampling error, while in the second case the conclusion would be that, although there was considerable overlapping, the functions measured by the two instruments were by no means the same, and that therefore the objectively

scored test was not a good measure of ability to reason in terms of the subject-matter taught.

The logic involved in the problem here raised might seem, perhaps, too obvious to require this analysis and discussion. This is hardly the case, however, in view of the frequency with which otherwise carefully conceived studies have been published in which the issue was evidently not recognized. I refrain from listing any such studies, since no useful purpose would be served thereby and students familiar with the experimental literature will easily supply instances. The principles involved when once recognized are easily applied. In all studies where overlapping or "true" correlation of functions is in question, the "split-test" method should be avoided and the "equivalent-forms" technique used instead. This can usually be done without appreciable additional labor where the measuring instruments are constructed specifically for a given study. If standardized tests are used, only such tests as are available in two or more forms should be used.

The issue itself is worthy of experimental attack and should not be decided upon the basis of *a priori* logic, whatever the apparent cogency of such logic. There are frequently studies made of already collected data where the only recourse in determining reliability is some variant of the "split-test" procedure. It will therefore be useful to know the experimental differences possible for defined populations and measures.

The well-known decrease in the correlation of mental measurements with increased time[2] between testings is rather closely related to the present issue. The "equivalent-forms" technique of determining the reliability obviously involves the introduction of a time interval, however short, between successive forms. It is likely that with learned responses of many sorts a relatively short time interval may be of considerable significance with respect to obtained reliability, such that a matter of hours or days might produce differences comparable to differences in intelligence tests involving intervals of months and years.

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[2]See, for example, Brown (1), who summarizes data from a number of studies.

INDIVIDUAL DIFFERENCES IN THE EFFECTS OF MESCAL

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Although a number of reports concerning the psychological effects of mescal (*Anhalonium* Lewinii) have appeared within the last forty years, in no case, as far as the present writer has been able to learn, have a number of subjects been given the drug at the same time and a comparative study made of their reactions to it. It is true that Prentiss and Morgan (5) as early as 1895 studied the physiological action of the drug and reported observations based on five subjects, one of whom was administered the drug twice. All of the five subjects experienced the unusual type of imagery characteristic of mescal but great individual differences were noted in the vividness, intensity, saturation, complexity, and duration of the visual phenomena.

The material about to be reported represents the by-product of another experiment, and, as such, is lacking the completeness which might be expected of data resulting from an experiment designed primarily to reveal individual differences in the effects of mescal. However, because of our relatively scant knowledge concerning the workings of this drug, it seems worth while to publish all available observations concerning it.

In conjunction with an experimental attempt to produce artificial chromaesthesia by the technique of the conditioned response,[1] five persons, including the writer, took doses of mescal in the hope that it might facilitate any latent tendency toward chromaesthesia which might have been established during the course of the experiment. Since each subject took the same dose and at the same time, and each kept a record of his experiences, it is of interest to note the individual differences appearing in their reactions to the drug.

Inasmuch as Klüver (4) has already published an excellent account of mescal and its characteristics, it is not necessary to include here a complete description of the drug or to review in detail its many interesting properties. However, in order that the reader may be in a better position to evaluate the material which follows, it seems desirable to state briefly some of the more important facts concerning this so-called "Divine Plant."[2]

"Mescal Buttons" or mescal beans are the dried tops cut from several species of the cactus Lophorus (*Anhalonium*). The drug has been variously named by different groups of Indians who use it in connection with certain of their religious ceremonies. It has been called mescal in practically all of the English scientific writings concerning it. Commercially it is known as "peyote" a derivative of the Aztec word "peyotl" which is an Indian word for "cocoon." The chemical constituents of the drug

[1]The manuscript of this article is now in press.

[2]This characterization seems to have been used first by Havelock Ellis (1).

are somewhat uncertain, although its active principle is conceded to be an alkaloid.

When ingested in sufficient quantities an unusual train of phenomena results. The chief physiological effects are a lowering of the pulse rate and a state of indigestion often accompanied by headaches. The psychological effects are much more interesting, and, happily, more pleasant. The most predominant of these is the production of colored imagery, kaleidoscopic in nature, and varied beyond description. All colors, forms, shapes, and movements imaginable appear as hallucinations upon closing the eyes or upon entering a dark room. Another marked psychological effect is hyperaesthesia of certain senses, especially vision. Small bits of faded color are seen as brilliant splashes; faint sounds are unusually noticeable; and normally unobserved kinaesthetic sensations are so marked as to be annoying. A very marked hyperaesthesia is noted for the specific odor of mescal which, when one is not under the influence of the drug, has only a faintly perceptible scent.

Still another psychological effect of the drug, especially when ingested in large amounts, is a temporal and spatial disorientation (2). Because one experiences thousands of sensations which are normally below the limen of consciousness, time seems greatly extended. In some cases the perception of space is also distorted as it is by hasheesh, and objects often appear much larger or smaller than they really are.

These, then, are some of the experiences to which the five above-mentioned subjects might look forward. They will be referred to as Subjects A, B, C, D, and E (the writer). All were students of psychology and all were between the ages of twenty and twenty-five. Subjects A and B were female and the other three were male.

Although standard doses of the drug have not been determined, the reports of previous observers indicated that six buttons, or approximately 15 grams, would probably be sufficient to produce the imagery, and yet not enough to be at all dangerous to life or health. The buttons were mashed with motar and pestle in order to make ingestion reasonably easy and were then swallowed with the aid of water.[3] Three doses of 5 grams each were taken, each with about 8 ounces of water.

The first dose was administered[4] at 2:10 P.M., at which time each subject

[3]After the first dose there is a hyperaesthesia for the specific and distinctly unpleasant odor of the drug, which often makes ingestion very difficult. The writer suggests that future experimenters take their doses in capsule form rather than as here described. It is possible that this method would serve to ameliorate the unpleasant physiological reaction of the drug without affecting the desired psychological reaction.

[4]I am indebted to Professor W. R. Miles of Stanford University for providing the mescal buttons and to Dr. Victor Hall, also of Stanford, for medical supervision of the experiment.

was provided with a notebook and instructed to keep a record of his experiences. At 3:05 all subjects entered a dark room and the C-major diatonic scale was played on a small piano-keyboard accordion. The second dose was administered at 3:10 P.M., followed by a second presentation of the tonal stimuli at 3:45 and a demonstration of the phi-phenomenon by means of flashlight bulbs. The third dose was given at 4:10. Subjects D and E also took a fourth dose of 5 grams at 5:10 and at 6:30 respectively.

All but one of the subjects were rewarded for their pains by gorgeous arrays of colored visions which vied with those described by previous investigators. For all but one, however, the shows were expensive, since all but the writer suffered from severe attacks of nausea. Unfortunately, Subject D, who did not observe visions of any kind, seemed to suffer more than any of the others. One is here reminded of the *Katzenjammers* suffered by William James (3), when he sought to make a personal study of the drug.

Let us now look at the records made by the five subjects while experiencing the effects of mescal. It is to be regretted that it is not possible to reproduce them in full, since in many cases attempts were made to draw the imagery experienced. However, the following excerpts from the records, arranged in a temporal sequence, present a fair picture of the reactions experienced by the various subjects and at the same time serve to emphasize the likenesses and differences in them. In all of the notebooks, including that of Subject D, who did not experience any unusual imagery, there is evidence of a marked lowering of motor coordination as indicated by a constantly decreasing clarity of handwriting, so great in some cases as to make it practically illegible. In reading the following excerpts it should be remembered that they were written by persons in a distinctly abnormal state of mind; otherwise, some of the remarks might seem very peculiar. They have been copied, practically as written, without any attempt to give them coherence or to correct grammatical errors.

Two additional points should be emphasized: First, that none of the subjects were trained in the technique of introspection. They merely tried to make some record of what they were experiencing. Secondly, and this is of even greater importance, it would be impossible for anyone, no matter how well trained, to describe, even orally, all of his experiences because of their profusion, their strangeness, and the rapidity with which they change. This inability to report and describe more than a fraction of what is experienced has been mentioned by every previous observer. Even Fernberger (2), who dictated his observations to a stenographer, found it impossible to describe all of his experiences. In the case of the present records, which were of necessity written by the subjects as they came in and out of the dark room, it is obvious that the accounts are woefully incomplete. Indeed, in many instances it will be seen that the entries represent merely

an attempt to summarize the experiences occurring in the preceding period of time.

With these reservations in mind, let us now proceed to an examination of the records. For the reasons already mentioned, the accounts of the individual subjects are presented not as units but have been dissected and rearranged in a temporal sequence. The figure on the extreme left shows the time of the notation (each subject had his own watch), while the letters A, B, C, D, and E indicate the subject making the observation.

2 to 3 o'clock

A	2:10	Mescal has a slightly bitter alkaline taste (somewhat like acetanelid). The taste persists for a short while after taking the drug—around ten minutes.
B	2-3	Writing a term paper.
C	2-3	Taking an examination in neurology.
D		No comment.
E	2:10	Rather bitter nauseous taste—perhaps a little like the taste of hay or ragweed.
A	3:00	I feel a slight "weaving back and forth sensation."
B	2:40	First begin to feel a strange calmness.
D	2:50	Slight ache or pressure in or above eyes.
E	2:50	A vague—almost imperceptible pressure sensation in temporal lobes.

3 to 4 o'clock

A	3:05	As the scale was played I saw a pale light which wove back and forth seemingly with the vibrations of the notes. The light was of no special color—just ordinary daylight slightly cream-colored.
B	3:00	Before scales—a blue triangle, with faint yellow background changing to red. Now mostly red sometimes shading into orange-yellow or into violet.
C	3:05	Walking downstairs, felt like my calves were weak. Head feels dull and stomach not any too good. Feel as if I had been in a swing. A violet caudate nucleus to the wall and corner of ceiling. Haziness as notes are played.
D	3:05	No colors seen in dark-room. Note E guessed same as one heard 1000 times yesterday—actually note A.
E	3:05	Upon entering dark room—a very slight purplish tint in extreme right periphery of visual field.
A	3:10	Have a slight headache in upper occipital region. Rather a dull lopsided sort of pressure. A cigarette is normal in taste.
B	3:20	Started writing letters. Feel slightly excited and slight waves of numbness if I stop to think about it.
C	3:10	Two more buttons. My stomach—Oh! How can I take two more buttons.

D 3:10 In dark-room. Feel restless, especially in legs.
Positive after-images yellow just like light bulbs.
Same thing yesterday with blue lamps.

E 3:25 The dull pressure in the temporal lobes has become more general and a slight headache is noticeable.

A 3:40 Went into dark-room. Feel slightly "stewed" also a slight nausea. Everyone but me seems rather silly.
B giggles all of the time.

B 3:35 Woozy to walk. People seem to talk slowly.
A little round cell, blue with pink center and green border—now all a pinkish purple.
Blue field with 3 pink flowers with yellow centers, changing to pinkish-blue or blue with pink center.

C 3:35 Concentric circles of blue and purple.
Regular Fourth of July fireworks, white lights began falling then violet, blue and green. Moved down to the floor and under the chair. As I looked up—two equilateral triangles, one with blue balls at each corner, the other with green ones at the same place.

A 3:50 Oh—I'm sleepy. Writing is rather difficult.

B 3:50 Very apathetic—others are too.
Phi-phenomenon as usual. Afterwards, a blue triangle, then a violet and a green one. Bubbling paint pot splashing out concentric rings of violet and green of 2 shades.
Feel cold.

C 3:45 Two violet blue eyes, one in each corner of the room.
Change—pronto—green egg in the middle, hazy at first and then it came toward me.
Everything but my stomach seems funny.
Phi-phenomenon was terribly irritating.
On looking out of window, note that dark lines are violet and spaces are blue. Note too that I am having an awful time with spelling.
Fish with lurid sparkling eyes—violet and green scales.
Even as I write, I can see violet behind my eye lids.
God help my stomach—it is near the third button.

D 3:50 No spontaneous images. No unsteadiness in writing noticeable.

E 3:45 After-images of light bulb usually brilliant.

4 to 5 o'clock

A 4:10 Third dose was horribly nauseating. The mescal has a decided odor now where it didn't have before. Motor co-ordination is not so good.

B 4:10 Finished taking about 1 button when lost most of all taken before—a light purple and yellow windmill. Then the whole field filled with scribblings. Amphitheatre of four tiers, each going in different directions—light blue. Then suspension bridges—*ad infinitum*.
Strings of all colors. Beads—yellow-green. A giant spider web. Huge flower made of blue china places. Colored plumes. Rainbow spirals.

C 4:10 Taking that third dose was harder for me than anything I ever have done. If I can only keep it down.
My eyes at present feel as if they were turning up and in. Paper is turning greenish, now back to colorless again. Have a terrible headache. Have body sway when I try to walk. Have green outlining my fingers. Everything on outside of my vision is green.

E 4:00 Odor of mescal very noticeable and nauseous for first time.

A 4:40 I feel nauseated. Geometric design of light color—a slight tinge of green is noticeable through—later same design with dots of blue. Now a curtain—descends and rises gradually. I I feel awfully nauseated.

C 4:22 Eating green mint. It tastes like spinach with a ticklish flavor. Gee—the time is going slowly. I feel nervous instead of calm, feel like doing something, dancing or anything. Face feels terribly hot.

4:30 On looking out of window—most beautiful violet clouds. Feeling like sitting here forever—much calmer now. Paper a beautiful violet. Oh—feel so tired. Ain't we kids? Concentric circles—awful dirty red.

D 4:50 After ten minutes in the dark—first feeling of nausea. About 30 seconds later, a very slight rhythmical lighting of black field in periphery.[5]

E 4:45 Motor co-ordination badly upset—very difficult to write.

4:48 Green fir tree—like an after-image. Spiderweb spinning in a clockwise direction.

5 to 6 o'clock

A 5:00 Bird's nest with 4 eggs in it and after a little they hatched into blue forget-me-nots. Worm with green and blue on edges. Jagged flowers on end of the worm. Gold bands like canals. Moist hands—saliva normal. Saw a katy-did. Long line of nude ladies with long eye lashes. Butterfly with eyes and changing colors. Cats coming down. Background hard to define, but figures perfectly distinct.

B 5:05 Feel rather numb. Awfully hard to think of words for things. Hard to comprehend what people mean. Flowered caterpillars, green and purple like printed calico.
First they were like cactus, then started to wiggle and move. Yellow corpuscles with red dots streaming in certain streams. Rainbow snakes, fishes and monsters seem to come out of nothing. Colored wires wriggling.
Merry-go-round on top of water turns into figures which sway back and forth. Monkey faces with rainbow halos—rainbow trout and other fishes in hordes. Balloons, embroideries, dancing daisies, 2 hippotomi kissing. Halo flashing different faces each time.

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[5]This was the sole experience on the part of Subject D of anything that might be called unusual visual imagery.

C 5:00 Snake of violet and scarlet red with eyes closed.
Stomach is very unsettled—feel as if I had a lump in my stomach. Skin feels (gosh—but that word is getting me) dull. Huge red lips with white teeth shining through.
My face is burning up.

D 5:10 Seventh and eighth buttons. Strong impulse to vomit, which disappeared as such after two or three minutes.
No colors in dark.

5:15 A new wave of nausea.

E 5:15 Green chipmunk. Large yellow tulip—shot like a cannon releasing a gorgeous fountain of colors.

A 5:30 (Could no longer write. Following written by fellow student who was observing the experiment.) Musical scales produced imagery of cones, concentric circles receding to vanishing point, revolving clockwise, faster with higher notes. Pine trees. Corpuscles of color, frogs, snakes. Restless—secretions increase. Colors appear bright. Feel like running after all colors. First saw red stripe in a suit. See distant objects clearly. Inhibitions loosened. Red line around letters at first glance. To read is nauseating. Swallowing loud and disturbing. Main desire—to get it over. Feel far from floor. Glance at colored cards—colors are alive—when eyes are closed colors seem to pulsate. Notice red quickly. Upon closing eyes one enters another world; one forgets what he has just spoken; coherence suffers; speech is an effort.

C 5:30 Hanging out of window—so sick. And keep seeing a parrot that's red with green beak. Sombrero with blue beads for trimmings grew into wigwam with blue beads around flap door.

D 5:45 Another very extreme wave of nausea.

6 to 7 o'clock

C 6:00 Everything is in shades of purple and they are running around. The clouds out of the window seem to be made up of islands of purple. A cathedral window which I saw previously had beautiful individual designs—blue squares inside of circles of orange. White triangles inside of plack polygons and they were all so clear and distinct and fitted together to form an immense window.

D 6:15 Vomited. Looked deathly pale according to others. Then lay on a couch for about an hour and felt better but quite weak. Never any images or colors. Cold sweat.

E 6:30 Fourth dose—5 grams. Very nauseous to smell and could hardly swallow it.

7 to 10 o'clock

A 7:00 Badly uninhibited at dinner. Cup of chocolate seen as pond and spoon as green frog jumping in pond. Extremely humorous. Great difficulty in walking.

B 8:00 Home. Tastes are insignificant. Sugar and salt have no taste at first—then slowly appearing. This perhaps due to dryness of mouth. Cracked wheat bread has lost most of its flavor. Cookies taste faintly sweet.

Pimento cheese, very mild. Fish salad—repulsive.
Ate two pieces of bread and cheese, toasted, a piece of bread and a cooky. Aside from lack of taste—not repulsive. Things are hard to do, e.g., cut bread and put away things.

C 8:00 Still very nauseated but feel sane. Purple still covers everything.

8:20 No more purple—see colors naturally now.

E 7:00 To dinner in a cafe with Subject A and an observer.
Quite uninhibited—acted as if drunk. Ordered a small steak, french fried potatoes, lettuce salad and coffee.
Foods tasted only as they first touched the mouth, then became flat tasteless masses—must chew for a long time and even then swallow only with effort. No taste of salt.
The steak looks simply magnificent—extremely large and so prettily set off by bright green lettuce leaves.
Not hungry—eating seemed unessential as compared with visual experiences. Tail lights on cars and neon signs simply fascinating. All colors, especially red seemed to have an hypnotic effect. Eyes constantly searching for colors. Tiny colored threads in clothing stand out so sharply that cloth appears striped. All noises greatly amplified. Small noises (rattle of car) sound like breaking glass. No nausea—extremely happy. Continual kaleidoscopic visions—especially with eyes closed. Predominated—a whirl of incandescent globules or jewels of every color and description—sometime moving in one direction and sometimes in the other. Jewels hanging on ceiling extending indefinitely into space. Time greatly exaggerated. Cried and laughed on slightest provocation.

B 8:30 Drove back to laboratory. Driving is exhilirating. I have good control. Colored things stand out somewhat more than usual.

9:15 Into dark-room. Visions persist. Playing scales or notes does not change them. After-images are beaded. E swings a cigarette and we see a ball of fire following it.
This convulses us, especially E. Later—drove home and tried smoking a cigarette—very mild. Inhaled and felt woozy. Tried to read but it is very slow. Things seem funny.

E 8-10 Smoke curtain of scarlets, reds and violets. The drinking fountain appears as a flowing rainbow. Sense or illusion of general hyperaesthesia. Chromatic aberrations of white light greatly over-emphasized. Many images of the general nature of coiled tubes with small particles of all colors moving in all directions. Violet predominating color, but all colors are seen.

Remainder of the Night

A 11:00 To bed, but could not sleep all night because of vivid visual imagery and the loudness of ordinarily unnoticed sounds. Bad stomach pains which seemed beautifully colored and which became brighted in color as the pain increased. A very restless night.

B 11:30 To bed. Stayed awake until twelve-thirty seeing gorgeous colors, floral designs, beaded dresses, embroidered runners, colored balloons, fires, dancing flowers, etc.

C 12:30 Had to get out of bed to get my note book for no sooner did I get in bed than I started seeing colored things all over again. Saw the most striking Japanese paper-prints, the colors changed, first a bird of paradise—the tail feathers changed to green then red. Then Fujiama only the snow part was in baby blue. I certainly never heard the clock strike so loudly. Noises just as I imagined always that ghosts would make when I was a child. Lights off.

1:15 I am now sure that the noises were in me and not in the room or outside. Rasping clicks—now the sound of rushing water. Lights off again.

1:45 Had to turn off lights again. My stomach has seemed like a bubbling cauldron—it feels as if it were radiating heat. I never felt anything so peculiar. Lights off.

D 11:45 To bed and to sleep about twelve-thirty. No images at any time. Slept soundly until 7:30. No dreams remembered.

E 11:00 To bed. Could not sleep until three A.M. because of imagery on closing eyes and hearing of many ordinarily unnoticed sounds. Definite synaesthetic tendencies. Imagined could see all parts of body by moving them. Extremely conscious of kinaesthetic sensation on moving joints. Also seemed to hear noises in my body.

Next Morning

A Next day—tired and still seeing imagery whenever eyes were closed. Slept next night as usual, but still occasional imagery on the second day after taking mescal.

B 6:00 Awake, felt pretty well.

7:30 Up, very tired. Breakfast tasted fairly normal tho' a little pale. Don't get visions any more. Not shutting eyes from light any more.

C 7:00 Woke up this morning feeling fairly good—not particularly tired but a little wobbly on my feet.

10:00 Got whirls of dirty red in dark-room. Nauseous at times second and third days.

E 6:00 Awake—no nausea, headache or images. Eye balls slightly tender. Occasional dull pain in head throughout the day. Taste normal. Smell unusually acute, especially odor of mescal in the room in which the experiment was conducted.

In spite of the relative crudity of the above records, it is evident that significant individual differences appeared in the subjects' reactions to the drug. Perhaps the most strking was the failure of Subject D to experience any unusual magery of any sort although he did experience the usual physiological reactions. Next should be mentioned the almost complete absence of a physiological disturbance (headache, nausea, etc.) on the part of Subject E although he took an extra five-gram dose of the drug. Nevertheless, he experienced psychological effects as markedly as any other member of the group. The third important individual difference which appeared was the radically different duration of the

effect ranging from the relatively sudden disappearance of the visual effect in Subject C to the recurrently appearing images on the second day in the case of Subject A. It should be mentioned here that Subject A shows a decided tendency toward eidetic imagery and also that she was in a rather fatigued physical condition at the time of the experiment.

The wide variation in the actual imagery reported by the subjects was of course to be expected. Perhaps the most significant observation here is the similarity of certain aspects of the imagery. All of the subjects reported blue or purple as the most predominant and most often re-occurring color and all (although some did not record it in writing) remarked about the prevalence of concentric circles (spider-web formations) in their visual forms. Also of interest is the fact that all of the four subjects reported seeing small colored animals of the "Mickey Mouse" variety, and it was suggested that this might be the result of each member of the group having seen a film of this type on the previous night. This explanation, in terms of recent experience, is hardly satisfactory since there was a marked absence of squares in the subjects' imagery, yet all had seen thousands of colored squares in the course of the conditioning experiment. The writer is more inclined to attribute such similarity of imagery to suggestion, since the subjects were together and were constantly discussing their imagery with one another.

Still another interesting effect of the drug should be mentioned: the markedly increased duration of the positive after-images previously noted by Prentiss and Morgan (5) and also by Fernberger (2). Although no attempt was made to study the phenomenon scientifically,[6] the notation made by Subject B at 9:15 in regard to the cigarette calls it forcefully to our attention. There was, however, no noticeable effect on the perception of the phi-phenomenon.

As stated at the beginning of the paper, the material here reported was only the by-product of another experiment. The varied observations secured from this group of five subjects may serve to emphasize the possible usefulness of mescal as an agent of research in the field of visual phenomena. The unpleasant physiological effects of the drug as well as the possible physical harm which may accompany its repeated ingestion by the same subjects tend to make its use somewhat precarious, but it does seem to offer unique possibilities along certain lines of investigation.

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[6]The writer was under the influence of the drug and consequently was in no condition to devise suitable elaborations of the experiment.

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THE INFLUENCE OF SOME ANTIPYRETIC DRUGS ON LEARNING*

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The problem concerning the influence of antipyretic drugs is one on which little previous investigation has taken place. Since these, and other similar drugs, are used by many persons at different times without regard to their effect other than relief, an investigation of the possible effect of these antipyretics upon the rate of learning is justified.

The effects of antipyretics were first studied by Münsterberg (9), and later by Macht, Isaacs, and Greenburg (7). Both studies were concerned with psychological reaction-times. Münsterberg found that antipyrin and quinine lengthened the time. The doses which he used, however, were exceedingly large. Macht, Isaacs, and Greenburg found that all the antipyretics with the exception of quinine retarded reaction-times. Therapeutic doses were given.

Lashley (5, 6), using strychnine and caffeine, reports that the former in small doses has little or no effect upon habit formation in rats. Large doses, however, give incoordination of movement but, at the same time, accelerate learning. Caffeine, on the other hand, in doses of 0.5 mgm. or more, retards the learning in direct proportion to the size of the dose.

The aim of this study is to investigate the effects of aspirin and quinine upon the rate of learning nonsense syllables.

There are two groups or types of antipyretics. The first of these is made up of aspirin, antipyrin, acetanilid, acetphenetidin, pyramidon, and salol. These are known as antipyrins. This type of drug causes vasodilation and directly increases the heat loss. The second group is made up of quinine and its compounds. These are distinguished from the antipyrins by lessening heat production by direct action on various metabolic processes in the tissues.

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Aspirin is the trade name for acetylsalicylic acid. It is a derivative of salicylic acid whose chemical term is ortho-oxybenzoic acid. It is used extensively for the relief of headaches and febrile temperatures. The ability of aspirin to lower febrile temperatures marks it as important in the antipyretic field. This is accomplished by an increase of heat dissipation through cutaneous dilation and increased perspiration.

Quinine is one of the most important, if not the most important, of the antipyretics. It is used extensively throughout the warmer climates where malaria and other tropical fevers are common.

Quinine reduces heat production by diminishing the nitrogen metabolism. Small doses of quinine cause a quickening of the pulse rate and a rise in blood pressure. Such a pressure rise depends upon vasoconstriction which is possibly caused by direct stimulation of the unstriped muscles in the blood vessels. This is brought about by cardiac depression; peripheral action accounts for the vasodilation.

Quinine produces some local irritation and anaesthesia. The latter is produced when the analgesic is in direct contact with sensory nerves.

The drugs were given in doses slightly larger than the minimum dosages as given by Sollman (10), but in quantities recommended by a registered pharmacist. The dosage for aspirin was 10.0 grains; for quinine, the dosage was 5.0 grains.

Capsules were used in administering the drugs. This method was chosen because of the ease with which the conditions might be controlled. The control condition was a capsule exactly the same as in the other conditions except that the contents were ordinary flour instead of the customary drug.

Lists of 12 syllables were used with each condition. These lists were interchanged so that all the lists would be learned approximately the same number of times under each condition.

There were three conditions: Condition A denotes learning under the influence of aspirin. Condition B denotes the use of quinine. Condition C is the control condition. The order of giving the conditions was scrambled so that any practice factors might be eliminated.

All drugs were presented 30 minutes before the learning began. This time was kept constant for all subjects throughout all conditions, including the control condition. This period was allowed in order to give the drugs a sufficient amount of time to become active.

At least 48 hours elapsed between each condition for each subject. That is to say, if a subject had Condition A on Monday, he did not have Condition B before Wednesday.

As far as was possible, two hours were allowed to pass between a meal and the administering of a drug. No appointments were made before 10 A.M. or 2:30 P.M.

The time of day was kept constant for each subject also. Although this

TABLE 1

	Av. no. trials	Standard deviation	P.E.
Aspirin	10.48	3.83	0.51
Quinine	10.28	3.92	0.52
Control	10.00	3.93	0.53

detail of control was not consistently constant, the variations were never more than one hour earlier or later.

The anticipation method of learning was used. One learning trial was given before the anticipation began. Recall was written by the subject.

Twenty-five subjects were used. They were all students in the University of Chicago. The group was almost equally divided between the sexes.

Results

1. The average number of trials to learn twelve nonsense syllables under the various conditions described above is practically the same (Table 1).

The differences in the average number of trials between the three conditions are slight and are not statistically reliable. The dispersions of the trials are almost identical under all three conditions.

2. A study of the composite curves for all three conditions shows that more syllables had been learned at trials 2, 3, 4, and 5 when quinine had been given than at the same periods for the other two conditions. These variations are not large enough, however, to be considered significant.

Conclusions

1. On the basis of the average number of trials to learn, quinine and aspirin may be said to have a neutral effect on learning.

2. The slight variations which appear in the number of trials are chance variations.

3. The early rise of the quinine curve may be due to a very short period of action on the part of the quinine but, more likely, it is caused by the entrance of some chance factor.

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A TRACE CONDITIONING OF THE GALVANIC REFLEX[1]

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This study was designed to investigate the establishment of short trace conditioned reactions in adult human subjects.

A trace conditioned response, like one of ordinary conditioning, requires three elements: first, a momentary substitute stimulus; secondly, a natural stimulus; and, thirdly, a response originally made only to the natural stimulus. It differs, however, in that the response is not conditioned immediately to follow the substitute stimulus, but instead appears after a definite interval of delay, this interval being greater than the normal latent period of the response.

In this experiment, the substitute stimulus was light from a 3.5-v. bulb. The natural stimulus was a rather strong electric shock. The response selected for measurement was the galvanic reflex in that this change in skin resistance follows a painful stimulus and is not readily susceptible to interference by conscious processes. Since, as is shown later, the galvanic reflex has a latency of approximately two seconds, five seconds was chosen as the time to intervene between the light and the shock.

The subject sat in a completely enclosed cubicle apart from the experimenter. The galvanometer electrodes were tightly bound to his left hand, while his right hand rested on an arm of the chair at the end of which

[1]From the Psychological Laboratory, Miami University. The writer wishes to thank Professor E. F. Patten for his aid in the making of this study, and Professor S. A. Switzer for the suggestion of the problem and continued guidance therein.

were two metallic finger rests. The shock was administered through these to the pads of his index and middle fingers. A variable inductorium was located in a room separate from both the subject and the experimenter. A wiring diagram is shown in Figure 1. The 3.5-v. light bulb was fixed on the wall facing the subject. It was approximately three feet away and on a level with his eyes.

Each subject was instructed as follows: "Place your fingers on the finger rests. Now just sit quietly and relax as much as you can. During the course of the experiment you will feel a mild electric shock. If at any time you remove your fingers from the rests, replace them immediately. Do not try to prevent any reaction you may tend to give. Keep your eyes open at all times. If there is a pause in the course of the experiment, just sit quietly; I shall tell you when it is finished."

Any change in skin resistance was shown by a deflection of a vertical beam of light reflected from the galvanometer mirror onto a translucent screen. The fluctuations of this beam were readily followed by a sliding pointer operated manually by the experimenter. A thread attached to the pointer ran through a 14-1 reducing device to a sliding carriage. An arm projecting from this carriage held a stylographic pen which graphically recorded on the polygraph paper any movement of the pointer (see Figure 2).

The period between the light and the shock was kept constant by an interval timer which made an electrical contact every five seconds. With a double-throw switch in one position, the light circuit was completed; with it in the opposite position, the shock circuit was completed. The experimenter shifted the switch from light to shock sometime during this constant interval. Care was taken to vary the time between pairs of stimuli. The completion of the light and shock circuits actuated magnets carrying stylographic pens and were thus graphically recorded. A vibrating interrupter with magnet and stylographic pen recorded time in one-sixth seconds (see Figures 1 and 2).

The subjects were all male undergraduates of the University, none of whom had taken more than an elementary course in psychology.

In the final[2] experiment, three groups of subjects were used, an experimental group and two control groups. The latter two groups will hereafter be known as the "latent-period" group and the "irradiation" group. From 15 to 30 paired stimulations of *light→five-second interval→shock* were given the 12 members of the experimental group. In all cases a change in skin resistance, shown by a galvanometric deflection, followed the shock. The latent period of the reflex tended to become smaller for each subject

[2]A preliminary experiment was run, using ten subjects. The results of this were not clear-cut because of a faulty recording apparatus, and hence will not appear here.

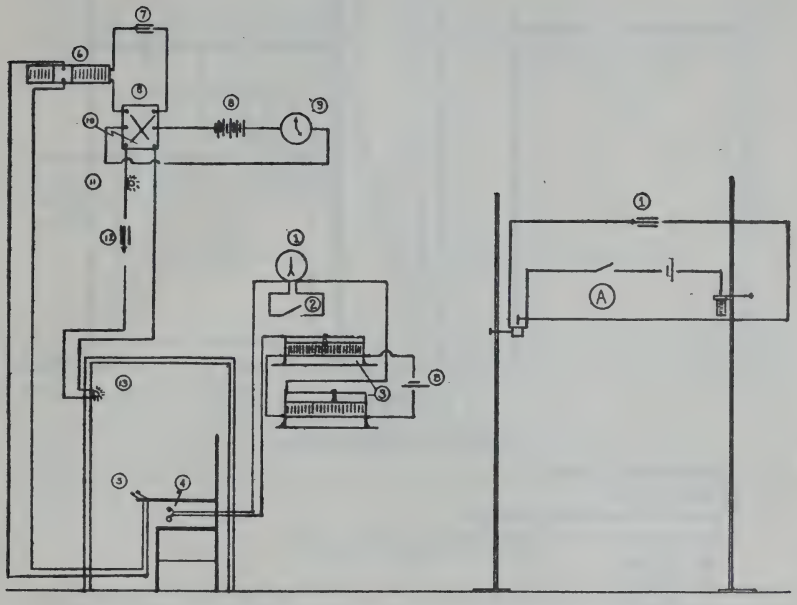

FIGURE 1

DIAGRAM SHOWING ARRANGEMENT OF THE APPARATUS

1. Galvanometer
2. Damping key
3. Resistance to control galvanometer deflection
4. Galvanometer electrodes in subject cubicle
5. Finger rests
6. Variable inductorium in separate room
7. Shock signal marker
8. Side of double-throw switch controlling light
9. Interval timer
10. Side of double-throw switch controlling shock
11. Pilot light
12. Light signal marker
13. 3.5-v. light bulb in subject cubicle
B. Batteries
A. Detail of vibrating interrupter
1. Interrupter signal marker

All markers *1*, *7*, and *12* were together on the polygraph paper. (See Figure 2.)

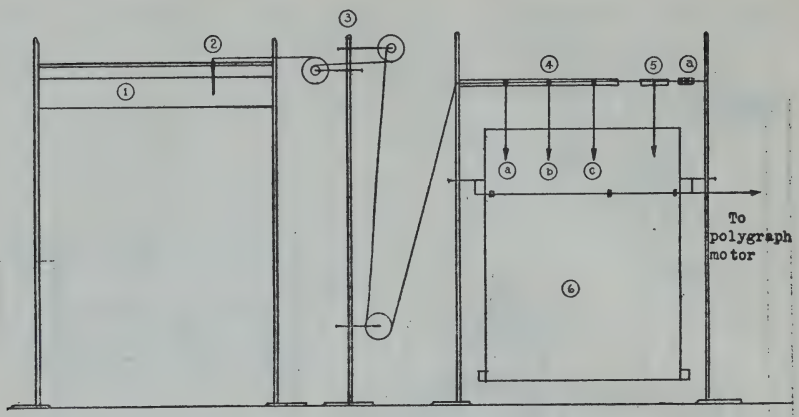

FIGURE 2

RECORDING MECHANISM

1. Translucent screen
2. Sliding pointer
3. Movement-reducing device
4. Signal markers carrying stylographic pens
   - *a.* Time
   - *b.* Light
   - *c.* Shock
5. Sliding carriage with arm carrying stylographic pen
   - *a.* Coil spring holding carriage so that movement of pointer was under a slight tension
6. Polygraph

as the experiment progressed. When the reaction began to occur almost simultaneously with the administration of the shock, the test series was begun. Herein the light was presented alone. If a change in skin resistance then occurred after a delay comparable to that of the training series (five seconds), the phenomenon of trace conditioning was indicated as being present.

Such a delay was found with six subjects, all of whom made at least one response approximately five seconds after being stimulated by the light alone. Subject Ko. gave four such responses and subject Su. gave three. The rest gave one only. A sample record is shown in Figure 3. Fifteen paired stimulations had preceded this response.

The top line of this record indicates time in one-sixth seconds. Any break in the next (second) line indicates the administration of the light stimulus. One such is shown at the left side of the record, at the point marked "X." It is to be seen that the light lasted approximately three-sixths of a second.

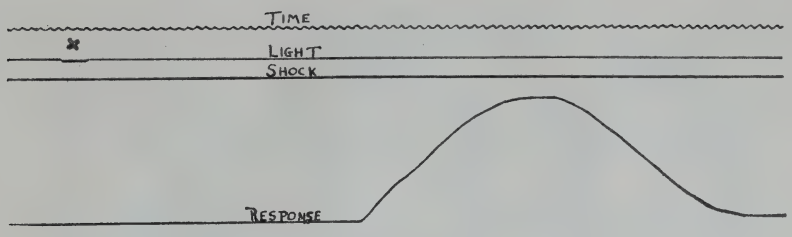

FIGURE 3
SAMPLE RECORD OF A TRACE CONDITIONED RESPONSE
(See text)

A break in the third line would indicate the administration of the shock. None appears here, however, since this is a test record. The bottom line is the tracing of the galvanometric deflection.

The results for this group may be seen in Table 1, column 2. Here it will be found that, while the responses varied between 31 sixths or 5.16 seconds and 40 sixths or 6.66 seconds, all show an appreciable delay, averaging 5.96 seconds.

LATENT-PERIOD CONTROL

How long is the latent period of the galvanic reflex? A group of subjects was run to determine this. With this group of six subjects, the two stimuli were almost simultaneous, the shock following the light by about one-half second. All subjects made a response to the light alone after from four to eight paired stimulations. The average latent time of this group was 2.05 seconds, as may be seen by reference to Table 1, column 6.

If irradiation is a time as well as a space phenomenon, i.e., if it will also "spread" in time, then these results may not be due to genuine trace conditioning. To check this, a control group of six subjects was run. These subjects underwent a training series with shock alone over a period similar to that of the experimental group. In the test series, the subjects were stimulated with light alone. A change in skin resistance occurred with all subjects except one who did not respond at all. For the remaining five subjects, 2.20 seconds was the average time between stimulation by the light and the occurrence of the response (see Table 1, column 4). It was evident that irradiation or a general lowering of the stimulus threshold had taken place. There was, however, no tendency to delay beyond the normal latent period.

From an examination of Table 1, it will be seen that while the irradiation control group had an average latency of 2.20 seconds and the latent-period control group had an average latency of 2.05 seconds, the experimental group averaged 5.96 seconds. This would seem to be a genuine

TABLE 1

Experimental group		Irradiation group		Latent-period group	
Subject	Time*	Subject	Time	Subject	Time
Mo.	40	Ho.	11	Rh.	12
Su.	31	Mu.	20	Wal.	11
Pai.	39	Ge.	12	Par.	16
Ko.	36	Ha.	10	Jo.	11
Sh.	34	An.	13	Wi.	11
Sw.	35	Was.	xx†	Vi.	11
Average	35.8	Average	13.2	Average	12.3

Experimental group average—5.96 seconds
Irradiation group average—2.20 seconds
Latent-period group average—2.05 seconds

*In all cases *Time* is the number of sixth-seconds which elapsed between stimulation by the light and the occurrence of the response.

†Average of irradiation group is for only the five subjects who responded.

case of trace conditioning in that the time involved in the experimental group is considerably greater than the normal latent period of the reflex.

All subjects in the experimental group were questioned at the close of each experiment in an attempt to ascertain to what extent symbolic processes, such as counting, entered in to bring about the delayed reaction. Only one reported that he had considered counting, but said he "decided not to." Several remarked about an increasing anticipatory dread that assailed them during the interval between light and shock. One reported, "After the light had gone off, and I was waiting for the shock, I could feel the blood rushing to my head." Another said the sensation was similar to that experienced before taking a final examination.

APPARATUS

A DEVICE FOR MEASURING THE SUBCOSTAL ANGLE[1]

C. R. GARVEY[2]

Ever since the work of Kretschmer (4) on the relation between physique and mental disease there have been repeated attempts to utilize the size of the subcostal angle or the angle formed by the lines of the lower costal margins as an indication of body type or as a datum of constitutional anatomy. Kretschmer himself says that the asthenic (leptosomic), as contrasted with the pyknic, is a lean man with, among other characteristics, "a sharp rib-angle" (4, p. 21). Since this angle is not included in his schedule of measurements, we must assume that its sharpness was judged from visual inspection. Sommerville (5, pp. 37 f.) on the other hand, attempted to measure it objectively. His technique is laborious and time-consuming. It is of questionable validity since he assumes the subcostal angle to be twice the angle made by the midline and the lower costal margin on one side, an assumption which cannot hold on account of the high incidence of asymmetry and deformity in the structure of the lower part of the chest. Also, his practice of determining the angle from measurement of the horizontal distance from the midpoint of the sternum-umbilicus line to the point where the horizontal encounters the ribs (costal margin) introduces chance for great error, as well as invalidity due to individual differences in the curving of the costal margin, especially as the latter recedes from the apex of the angle.

Wertheimer and Hesketh (7) use the subcostal angle in their study of psychiatric patients but describe their technique no further than to say that it is easy and simple. "The subcostal angle is a measurement easily taken which seems to be of great constitutional significance. It shows wide variations in persons of different types. This measurement is a simple method of obtaining an indication of typical chest forms and is in a way a barometer of the morphologic constitution."

These examples, coupled with the lack of a standard anthropometric instrument for measuring the subcostal angle, illustrate the necessity for such a device. The writer has been forced to devise an instrument for this purpose in an attempt to check the relation between morphology and certain

[1]This device and method was developed in the Psychology Laboratory of the Institute of Human Relations in the course of an investigation of body build in mental disease being carried out under the direction of Dr. E. S. Robinson.

[2]National Research Council Fellow in the Biological Sciences.

psychological "types" (2). Since the original design required a large amount of study and exploratory construction, its description may save time for other investigators as well as introduce to them a method now in use for the measurement of this angle. It consists of a hinged metal angle fitted with a spring and friction attachment which causes the device to maintain any angle imposed upon it. The adjustment is such that the device can be easily set at any angle but will not collapse under its own weight or the forces of necessary handling. The device is applied edgewise against the subject's epigastrium with the hinge pin just below the end of the sternum. The instrument is then raised and its branches spread apart till they encounter the lower costal margins, i.e., till the instrument just fills the apex of the subcostal angle. It is then removed and applied directly to a protractor, or else the angle is drawn on paper to be measured later.

Except for difficulty in drilling the spring, the device is easily constructed. It is made from a simple 5-inch strap hinge, a piece of clock spring, 1 cm. wide and 11 or 12 cm. long, and a very small flat-head machine screw with two burrs, asembled as shown in Figure 1. Suitable hinges made of white metal, which is preferable to black iron, can be obtained in most dime stores. A small hole is drilled 3 or 4 cm. from the end of the slotted strap and a similar hole 1 or 2 cm. from one end of the spring. The other end of the spring is inserted into the slot in the strap from the inside and the spring forced into position and bolted. The head of the screw is countersunk from the outside of the hinge and the extra burr is added as a jam nut. The hole in the spring is 7 or 8 cm. farther from the long end of the spring than the corresponding hole is from the center of the hinge

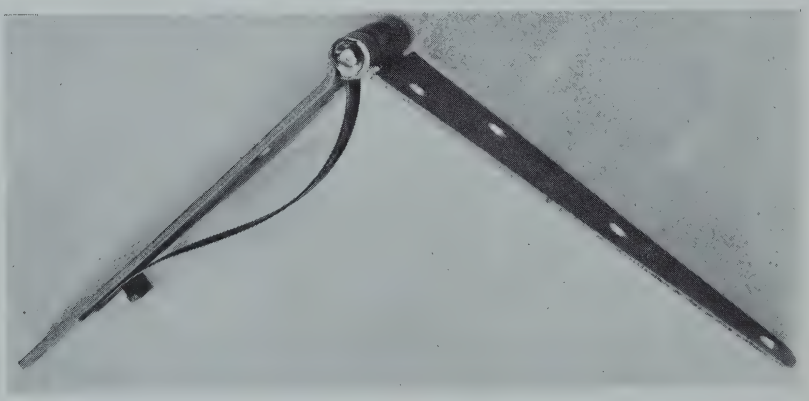

FIGURE 1
A Device for Measuring the Subcostal Angle

pin, to allow for the curve in the spring. The straps, whose surfaces are ordinarily tangential to the circumference of the pin bearing, are both bent very near the pin, so that their outer flat sides, if projected, would pass exactly through the center of the pin. The apex of any angle the hinge may describe is now at the center of the pin. This is not necessary and the straps need not be bent (1) if the device is never used to determine an angle from three points but always from two lines and (2) if the angle is never taken directly from the instrument with a protractor but always from paper after having been traced from the instrument. The latter method is probably more accurate, it saves time, especially during anthropometric examinations, since application of the protractor and reading of the angle can be deferred till any time after the examination, and it has the further advantage that it leaves a permanent record of the angle on the data sheet.

This instrument has been used on between two and three hundred subjects during the past two years and has proved satisfactory in many respects. On 55 of these subjects measurements were repeated on succeeding days and it is therefore possible to compute reliability measures. With 27 manic-depressive patients the mean discrepancy between measurement and re-measurement is 3.9 degrees, disregarding signs, where the range of angles

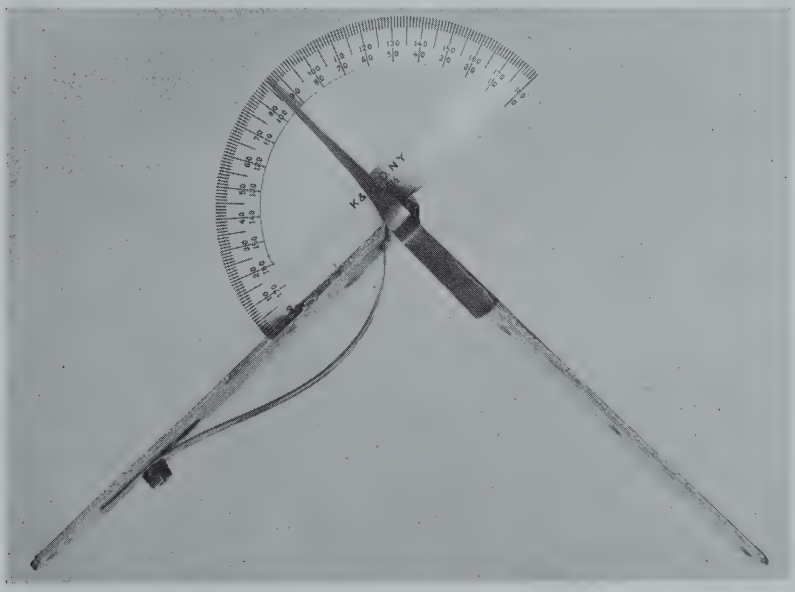

FIGURE 2
THE DEVICE WITH PROTRACTOR ATTACHED

is from 60 to 101 degrees and the mean 82 degrees. The Pearsonian coefficient of correlation is .89. With 28 dementia praecox patients the mean discrepancy is 4.4 and the reliability coefficient .80. On all 55 of these patients taken together the discrepancy is 4.16 and the coefficient .85.

The fact that the reliability is better with the manic-depressive than with the schizophrenic patients, who are less cooperative, indicates that with normal subjects or recovered patients we might expect to obtain results comparable in reliability with other physical measurements on normal living material (6, 1, 3). Reliability should also be increased by taking the measurement with the subject in a supine position, as suggested by Sommerville, but this was not required of our patients on account of the mental condition of many of them. In addition, the writer believes that methods of applying this instrument could be devised which would be more accurate and reliable than the one described above. For instance, the lower costal margin on each side might be located by palpation and marked with a line on the overlying skin. The instrument could then be approximated to the angle without pressure. With some sacrifice of simplicity in construction and perhaps of convenience in application, it can be provided with a scale for reading the angle directly, as in Figure 2.

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BOOKS

ALEXANDER R. LURIA. *The Nature of Human Conflicts.* New York: Liveright, 1932. Pp. xvii+431. (Translated from the Russian and edited by W. Horsley Gantt.)

Rarely does one find a problem of an experimental nature amenable to persistent investigation over a seven-year period with the use of but a single specialized technique. That such can occur is either an indication that a fundamentally fruitful method has been unearthed or that the procedure and apparatus themselves have served to frame the problem. A transition from the first to the second is not an impossibility; the reader of this new book out of the U. S. S. R. will perhaps be inclined to the view that such is the developmental course of Luria's work.

The experimental psychologist, familiar with the history of almost complete failure that has attended constant efforts to find reliable and useful indicators of the presence of emotions, is inclined to approach with caution any new claim that affective disturbances are possible of detection by instrumental methods. Such a claim is set forth in Luria's book. However, the critical reader is disarmed early in the examination of the volume by the realization that the methods employed do not consist simply of extravagant variations of old procedures. Luria has attempted something far more exciting and interesting than the observation and measurement of superficial by-products of emotion. He has been able to bring into close association two voluntary responses. Both, under the conditions of his experiments, are language mechanisms, one verbal the other gestural. The experimental subject is required to respond promptly to each of a list of words, in the manner of the conventional free association test, with the first word that comes to mind. Simultaneously, he is to press with his right hand a bulbular device which is connected pneumatically to a needle recording on a kymograph. The left hand, meanwhile, is engaged in maintaining a weight at a given level; it is otherwise passive and few results of its behavior enter into the interpretations of the author.

It is perhaps not at all obvious how such a technique ever yields evidences of internal conflict. The manual and verbal records, an amazing number of which are reproduced in the book, speak for themselves. When the stimulus word touches upon an emotional conflict or has emotional significance for the subject there results a temporary disorganization of manual activity. Poorly controlled, diffuse, and badly coordinated responses occur. To use the author's expression, a "disproportionately large amount of excitation is mobilized for use in the response"; the subject reacts "by dispersion." When the subject checks his first impulse to respond verbally

and casts about for a less "revealing" word, the manual response becomes dissociated, energy is released into the motor system and the hand writes what may be termed his "emotional autograph."

Professor Luria has been particularly fortunate in having access to a wide variety of cases for clinical study. Unlike the academic psychologist, who too frequently has to rely upon entirely artificial situations with the hope that emotional states will be induced, rarely getting more than transient phenomena, Luria has been able to experiment upon people who, due to the difficulties confronting them, could hardly fail to react in a genuinely emotional manner. As Professor of Psychology at the Academy of Communistic Education and researcher in the State Institute of Experimental Psychology in Moscow, he has access to suspected criminals, examining them only a few hours after their apprehension, convicted criminals, students in the Russian universities just before and just after the harrowing university "cleansing" examinations, subjects in hypnotic states, and many kinds of mentally defective and neurotic individuals. In all cases the results have shown a relative disorganization of behavior where emotional disturbances were present as compared with reactions to non-emotional stimuli.

On the side of neural interpretation Luria makes much of his concept of the "functional barrier." It is the latter which serves to restrain excitation from becoming transferred into the "motor sphere." Reduced to a shrewd neurological guess, it seems to designate a cortical inhibitory mechanism. Normal non-emotional adult behavior is characterized by a perfect working of the barrier; the behavior of the child differs in that he mobilizes a disproportionately large amount of excitation to be used in the response to a given stimulus, then evidences an insufficient functional barrier in that energy floods over into response channels, producing reactions "by dispersion." Again, the neurotic differs in that there is brought to bear upon the response an inadequate amount of excitation; in some forms of neurosis there are, in addition, barrier disturbances. In all cases emotional disturbances have the character of replacing normal responses with diffused reactive processes that tend to break down the functional barrier and find immediate motor expression.

The reader with special theoretical interests will find himself rewarded. There is possible, for example, a sharp and striking contrast of the analyses of Luria and Pavlov. The former shows a dependence upon neural dynamics of the Gestalt variety; his ideas with respect to the cortical inhibitory mechanism, for instance, do not involve a machinery as morphologically and functionally simple as that invoked by the Pavlov school. Rather the whole matter is cast in neurodynamical terms. Luria's debt to Lewin is great. Another interesting approach to the book might be the translation of Luria's concepts into psychoanalytical terminology; the relation of the two is far from obscure.

It is very much easier to arouse and sustain interest in the conceptions motivating Luria's experiments than in the written account of them. Whereas the volume contains considerable food for thought, it is in no sense predigested food, nor is it prepared in an attractive fashion. The intellectual appetite becomes dulled by careless and unnecessarily redundant expression. The average reader, even though he be versed in the technical lingo of experimental psychology, will feel that he is getting nourishment only at the expense of accepting along with it an undue dosage of verbal medicine. The reading of the book is itself a procedure which may develop a conflict in the reader. Luria seems always on the point of saying something of importance, a literary device which fatigues even as it stimulates interest. After struggling with its confused rhetoric for a time, one may find it necessary to turn to a more simply written piece to assure himself that the subjective symptoms of asymbolia are products of Luria's (or the translator's) style and not indicative of the onset of sensory aphasia. But it is perhaps the more kindly thing to do, and surely the more satisfactory to the reader, to view the book as an account of experimental findings. As such, it has much to commend it and, as such, it cannot fail to be more than mildly engrossing.

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RAYMOND HOLDER WHEELER. *Readings in Psychology.* New York: Crowell, 1930. Pp. xii+597.

The most conspicuous feature of this book is the Editor's notes preceding each reading. In these notes Dr. Wheeler seeks to influence the reader in favor of the organismic interpretation of human behavior. In the words of the author, "The point of view illustrated in these readings may be traced back to a vague and general beginning early in the history of psychology," . . . but it was more precisely the gestalt theory "that led to the interest in organismic principles in psychology, now developing in this country, where the term configuration is often used as equivalent of *Gestalt.*"

In the first reading Dr. Wheeler lays down eight "Organismic Laws" which he applies to the principles of social behavior. Following this, each reading, (presented apparently in decreasing complexity) is preceded by a separate note in which attempts are made to apply these so-called laws to the various psychological data. Often both facts and laws are strained considerably in order to bring them together. Space does not permit a critical discussion of the purposed laws and the organismic viewpoint in general. It should, however, be noted that the gestalt theory, from which the organismic theory has been derived, has at least the following ad-

vantages over the Wheelarian stand-point: (1) The derivation of its main assumptions does not rest entirely upon a fortuitous attempt to formulate a *new* system in psychology. A careful study of its publications reveals that its main concepts resulted from attempts to analyze experimental data on visual discrimination of temporal and spatial phenomena, especially the discrimination of movement. Previous concepts were considered inadequate to explain the observed phenomena, new principles were formulated and these were regarded as basic discoveries and as having general applicability in the entire realm of human behavior.

The organismic theory has no such experimental background except insofar as it is remotely related to the gestalt theory.

(2) The gestalt theory has marshalled together an abundance of experimental facts to support its underlying assumptions with considerable success. The organismic theory introduces numerous "laws" without providing for them ample experimental proof.

(3) The gestalt theory has constructed an interesting scheme for accomplishing a resolution of the controversy between vitalism and mechanism. By an analogy with the constitution of physics the theory has been organized into the province of gestalt statics and gestalt dynamics. This has led to an abandonment of the old distinction between sensory processes and higher thought processes. The organismic theory can boast no such detailed psychophysical scheme.

(4) The psychological conception in the gestalt theory is derived independently from descriptive as well as from functional analysis of the "phenomenally given." The Wheelarian theory rests primarily upon functional analysis.

Although the gestalt theory has numerous advantages over the organismic theory, one is often astonished at the generality of theoretical application in either case. Both theories invade psychological realms for which they fail to furnish empirical facts. Both theories often rely upon analogy for the establishment of purposed laws. This is especially true of the organismic theory. It would seem as if the whole situation in human behavior were suddenly clarified by a wholesale application of theories concerning the "whole situation."

Despite the close relationship which exists between the gestalt theory and the organismic theory which is supposedly characterized by these readings, there does not appear in this book a single reading by any of the chief exponents of the gestalt theory.

The book contains 28 readings which are proportioned as follows: I. Social Behavior, 4; II. Intelligent Behavior, 4; III. Emotional Behavior, 5; IV. Learning, 5; V. Reaction and Observational Behavior, 2; VI. Observational Behavior, 6; and VII. The Nervous System, 3. Thirty-five per cent of the readings are contributed by psychologists who are or

have recently been members of the psychology staff at the University of Kansas.

Group I furnishes a fairly representative sampling of issues concerning social behavior. However, such conventional topics as personality, character, moods, sentiments, etc., receive here only cursory treatment.

In the next group on intelligent behavior there appear several highly critical theoretical papers concerning the IQ and "intelligence as adaptation." Neither of the readings is chosen with the purpose of furnishing knowledge and understanding of the essential problems in this field. This group also contains a highly subjective paper on "Adaptive Behavior in Cats." It is essentially a repetition of Thorndike's work which, however, it does not equal in its adequacy of controls, straightforward objective analysis, or theoretical implications. The chief conclusion in this paper appears to be that through practice "the cat gets what it wants more readily than it did before, that is, with greater economy of movement." The author, Adams, however, does not explain precisely how and what the cat "wants" and what has modified the "getting."

The readings in Group III are captioned: "Goddard: A Case of Dual Personality," Wheeler: "A Case of Dementia Praecox," Cutsforth: "A Case of Retardation," Bagley: "A Study of Fear," and Richter: "Internal Drives." It is doubtful whether a general ballot cast by psychologists for representative literature on the emotions would include many of those readings.

Group IV contains readings on "Interest and Learning," "Laws of Exercise and Effect," "The Rôle of Form in Learning," and "Transfer of Learning." Although some important phases of learning are omitted here as in other sections, the contributors in this group write with greater authority in their respective fields.

If we evaluate *Readings in Psychology* in the light of such criteria as: (1) style, (2) elemental approach to basic problems, (3) factual and theoretical content, (4) clarity, and (5) classic in experimental technique, the last three sections (Groups V, VI, and VII) are undoubtedly the best in this book.

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